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THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. I.—*On the Genesis of Metalliferous Veins;* by JOSEPH LECONTE.

[Read before the National Academy of Science, April 17, 1883.]

THE phenomena of metalliferous deposit by solfataric action at Sulphur Bank and Steamboat Springs have tended strongly to confirm what I had previously believed to be the most probable theory of vein-formation, and at the same time to give it more clearness and definiteness. This paper, therefore, may be regarded as a continuation and development of the thoughts started in the previous ones.*

The structure, the mode of occurrence and the contents of metalliferous veins leave no longer any room for doubt that they have been formed by deposit from solutions. If any doubt still lingered on this subject, they are now dissipated by the phenomena of deposit still in progress at Sulphur Bank and at Steamboat Springs. Among metallic ores cinnabar has long been considered a possible exception to this mode of deposit. The extreme volatility of this sulphide, the extreme irregularity of its veins, and its frequent occurrence in the immediate vicinity of comparatively recent volcanic action, have suggested that it may have been deposited in irregular fissures, cracks, cavities, etc., by condensation of its vapors, sublimed by volcanic heat beneath. But the phenomena of Sulphur Bank and Steamboat Springs ought to settle this question forever. Cinnabar as well as other metallic sulphides are now being deposited there along with silica, from solutions. And yet so strong is the prejudice in favor of the sublimation theory for this sulphide, that Rolland after examining the phenomena of Sulphur Bank, after recognizing the complete saturation of all the rocks and earth with the up-coming solfataric waters and even truly describing the occurrence of the cinnabar in recently deposited silica in such wise that the two must have been deposited from the same solution—still talks vaguely about vapors and emanations of mercury.*

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* This Journal, vol. xxiv, pp. 23, 1882, and xxv, 424, 1883.

Admitting them as established, the view that metalliferous veins have been deposited from solutions, the most difficult questions still remain. What are the conditions under which deposit takes place? And what in addition to simple water have been the solvents?

Conditions of deposit.—In answering this first question it must be remembered that the chemistry of nature is far more subtle and refined than that of the laboratory—that substances which are regarded as practically insoluble in the latter cannot be so regarded in the former. The infinite patience of nature and the infinite slowness of her operation must be taken into account. In the perpetual circulation of subterranean waters infinitesimal deposits, continued and accumulated through almost infinite time, produce large results. Thus mineral veins may be composed of substances of extremest insolubility and yet be deposited from solutions. In fact such extreme insolubility, or at least very feeble solubility would seem to be a condition of mineral vein formation, for otherwise the materials would in most cases be brought to the surface instead of being deposited below.

Again, it must be borne in mind that solubility, even the feeblest, is notably increased by heat, especially super-heat, and by pressure. The effect of heat and especially of super-heat, in increasing solubility, is universally recognized; but pressure is usually regarded only as a necessary condition of super-heat, and not as itself an active agent. But, in fact, pressure acts not only indirectly as a condition of super-heat, but also directly as an active agent in increasing the solubility of nearly all substances. Mr. Sorby† has not only proved this by actual experiment on a great variety of substances, but has shown that it is a necessary consequence and beautiful illustration of the law of correlation and conservation of natural forces, and that we have in this as in the case of fusibility an example of the equivalency of mechanical and molecular forces. For as in the matter of fusibility, in all cases in which expansion takes place in fusion, pressure by resisting expansion, raises the fusing point, while only in those exceptional cases like ice, in which contraction takes place in fusion, pressure by assisting contraction, lowers the fusing point; so also in the matter of solubility: in all cases in which contraction takes place in solution—i. e., in which the volume of the solution is less than the combined volumes of constituents—pressure by assisting contraction increases solubility, while only in very exceptional cases, as for example sal ammoniac, in which expansion takes place in solution, pressure by resisting expansion diminishes solubility. These latter cases, however, are so extremely exceptional, that we may assume as a law, the increased solvent power of water in proportion to pressure. It is even possible by experiment thus to determine the mechanical equivalent of the chemical force of solution of any given substance, and in fact, this has been so determined for several substances by Mr. Sorby.

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* Annales des Mines, xiv, 384, 1878.

† Proc. Royal Society, vol. xii, p. 538, 1863.

There can be no doubt, then, that the solvent power of water may be increased without limit, by corresponding increase of heat and pressure. It is quite certain, therefore, that water deep in the interior of the earth, especially in volcanic regions, and therefore under heavy pressure and super-heat, would have its solvent power greatly increased, not only by the super-heat, but also by the pressure. It is believed that few substances could resist entirely its solvent power. Such waters coming up slowly toward the surface through fissures, large or small, would have their solvent power diminished, both by cooling and by relief of pressure and must of necessity deposit in their courses and form mineral veins.

Other solvents.—But the solvent power of subterranean waters is still further and very greatly increased for most vein-matters by the presence of alkali in the form of alkaline carbonates, or alkaline sulphides or both. This is especially true of the commonest of vein stuffs, viz: quartz and lime carbonate and the commonest form of metallic ore, viz: metallic sulphides. The solubility of silica in alkaline carbonate waters is well known, and with excess of carbonic acid in the waters all the earthy and metallic carbonates are also soluble. The solubility of many and probably all metallic sulphides in alkaline sulphides, especially with excess of hydrogen sulphide,* under pressure and super-heat can no longer be doubted; for iron sulphide and mercuric sulphide are now being deposited from such waters both at Sulphur Bank and at Steamboat Springs. Mr. Christy† and others have proved the solubility of mercuric sulphide under pressure and super-heat by actual experiment; and t are among the most insoluble of metallic sulphides.

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* The affinity of these two feeble acids for bases are so nearly balanced, that an excess of one involves the excess of the other, and as a matter of fact both CO_2 and H_2S are found in excess in all solfataras.

† This Journal, xvii, 453, 1879.

It is certain, then, that metallic sulphides are soluble limited extent in alkaline sulphides forming doubtless do sulphides. It is certain also that the solubility is increase superheat and pressure. It is therefore also certain that waters containing alkaline carbonates and alkaline sulphi circulating at great depth and therefore under heavy pres would take up silica, earthy and metallic carbonates, and tallic sulphides, and that coming up slowly toward the sur they would deposit these substances in theirr courses, pa by cooling and partly by relief of pressure, and thus f metalliferous veins.

We have given what seems to be the most usual caus deposit, viz: cooling and relief of pressure; but this is p ably not the only cause. There are many chemical react by which the same result may be attained. We will men only the more obvious of these. (*a*) Organic matters ar almost universal occurrence in subterranean waters, and t agency in reducing metallic oxides and metallic salts is known. The more we study the chemistry of nature, the n we are impressed with the importance of organic matter a universal reducing agent. Organic matter in form of hy carbons is almost invariably found in connection with cinna Its agency in reducing iron sulphate to sulphide is seen ev where. It is not improbable therefore that organic matter culating in the same solution with metallic sulphates may l frequent means of reducing these and depositing them as me lic sulphides. (*b*.) Again: alkaline carbonate and sulph waters, dissolving silica and metallic sulphides and comin contact in their course with decomposing organic matters, r be neutralized by the acids of organic decomposition and t compelled to deposit their freight. This apparently take deep hydraulic mines in the silicification of wood and possi in the deposit of iron sulphide. (*c*.) Lastly: it is possible t in some cases water from different sides and carrying diffe materials may meet in the same fissure and deposit by reacti For example, metallic sulphates thus meeting alkaline sulphi would by interchange produce alkaline sulphates and meta sulphides, which latter would be deposited. This seems to the reaction at the California Geysers where a black inky posit of iron sulphide is produced by the action of alkal sulphide on iron sulphate. If this reaction had taken pl sufficiently slowly, it is possible that the sulphide would h been crystalline. All these methods and perhaps many ot not yet imagined may occur; but the first, viz: by cooling relief of pressure is probably of most universal occurrence.

Such, we believe, is an outline of a true theory of the genesis of metalliferous veins, a theory apparently confirmed by the study of causes now in operation at Sulphur Bank and Steamboat Springs, and probably many other places in California and Nevada.* We are now prepared to still further confirm the theory by applying it to the explanation of all the phenomena of veins. But before doing so it is necessary that some grave objections recently raised should be removed.

Recently Dr. F. Sandberger has published a very important treatise on veins† in which he takes ground against what he calls "the ascension theory" and urges strong reasons, as he thinks, in favor of "lateral secretion" or "levigation theory." According to him metalliferous veins are filled not by deposit from ascending waters, but by lateral secretion or leaching from the immediate bounding walls of the fissures. He thinks that ascending waters or springs will not deposit in their conduits, but only at the surface in contact with air and by oxidation. He cites the case of iron ore which is thus abundantly deposited. He admits the deposit of vein-matters both silica and metallic sulphides, at Sulphur Bank and at Steamboat Springs, but thinks that these are only superficial deposits determined by contact with the air, that they are not true veins and throw no light on the process of mineral-vein-formation. He thinks that in case of solfataric waters depositing metallic sulphides, these waters in their ascent have traversed previously existing veins, a small portion of the metallic contents of which are dissolved and redeposited at the surface. For example: the metallic sulphides and gold found in the deposits of Steamboat Springs, according to him, were gathered from auriferous quartz veins in the neighborhood. He admits, also, however that the question might be definitely determined by deep explorations at these places, but is apparently unaware that such explorations have already been undertaken at Sulphur Bank. According to him, moving water will not deposit, for in order that deposit should take place the water must be stagnant. Therefore, for him, mineral vein-formation is always a deep-seated phenomenon, and we may never hope to see the process still going on, or investigate directly the causes still in operation. In brief his view seems to be, that the closed cavities of great fissures, like the closed fossil-cavities of stratified rocks and the closed vapor-cavities of eruptives are filled by leachings from the immediate bounding wall-rocks of the fissures. The process is a sort of aggregation of soluble matters into open spaces, an oozing and congelation of a coagulable plasma mending the broken parts. For such congelation-quiescence, stagnation seems to be a necessary condition.

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* Feeble solfataric action is still going on and probably still depositing metallic sulphides in Comstock Lode. (Becker.)

† "Untersuchungen über Erzgänge." F. Sandberger, Wiesbaden, 1882. I have not seen the full work, but only a thorough resumé of the part on genesis of veins by G. Maillard in the Archives des Sciences for Dec., 1882, vol. viii, p. 320.

The arguments which he urges for this view are mainly two. 1. That vein-contents both metallic and other, even in the same fissure, change with the country rock, as if derived immediately from its walls. 2. That by actual analyses of igneous and metamorphic rocks he finds in them all the matters, including the metals, contained in veins. This latter is his principal and as he thinks demonstrative argument; and his investigations on this subject are undoubtedly of very great interest and value. Previous investigators have also attempted to find the valuable metals widely distributed in the rocks, but with indifferent success because the analyses have been mostly undiscriminating analyses of the whole rock (bausch-analysen). Dr. Sandberger's analyses on the contrary have been selective analyses of the principal minerals. By this method he finds the valuable metals in notable quantities especially in the more basic rocks. Olivine, hornblende, augite and the dark micas he finds rich in a great number of metals, while the lighter-colored micas, feldspar, quartz and therefore the granites and gneisses, he finds poor. He thinks therefore that metals are derived mostly from igneous rock especially from the newer and more basic, and that veins are often rich when they intersect metamorphic rocks only because the metals are leached into them from the neighboring igneous rocks.

Such is a very brief statement of Dr. Sandberger's views. On account of his great ability and wide experience, they are certainly entitled to great weight. But it seems to me that all his objections to the ascension theory and all his arguments for the "lateral secretion" theory are based on a misconception of the former or rather on an exaggeratiom of the distinction between the two. The view which he apparently takes of the ascension theory is an extreme view which allies it with the vapor, or sublimation-ascension theory. According to the ascension theory, as he imagines it, the water comes up, and the materials are derived, from the unknown interior of earth—from the region of the sources of volcanic activity. This is plainly shown by his strongly contrasting it with his own theory as distinctively a levigation theory; also by the great importance which he attaches to his rock analyses as a crucial test for deciding between the two theories. Now I am quite sure that no one, in this country at least, holds to any such view. All speculators on this subject, I think, now hold that the mineral contents of veins are wholly derived by leaching, from the rocks forming the fissure-walls. The ascension theory (if we use this name at all) as properly understood, i. e., the eory which connects vein-formation with solfataric action is holly a levigation theory. According to this theory as I unerstand it, the vein matters including the metallic ores are not erived from an unknown mysterious region of volcanic fires, ut are gathered by leaching from the whole wall rock from top o bottom of the fissure; but mainly from the deeper parts, beause these are under heavy pressure and superheat. Subterranen waters gathering soluble matters from wide areas and great hicknesses of rock find their way into fissures, and these then ecome their natural channels back to the surface from which they come. The theory may be formulated as a depositing of materials, mainly in the return course of circulating meteoric waters. The materials are gathered widely by leaching, the deposit is mainly in the ascending portion of the course, because determined by cooling and relief of pressure. The only difference between great fissure veins on the one hand, and small infiltration veins, amygdules and geodes, etc., on the other, in their mode of filling, is that the latter are filled with matters contributed by the immediate bounding rocks only, while the former are filled in each part by matters gathered from all the rocks over wide areas and through the many thousand feet thickness through which the fissure breaks, but especially from the lower portion because hot. This is the reason of the great variety of the contents of these as compared with the other. In the one case the contribution is local, in the other more general; the one may be regarded as the gatherings of a municipal tax, the other of a national tax; but in both cases the tax is drawn from home sources, not from unknown foreign sources. Dr. Sandberger's analyses indeed proves that leaching is the source, but proves nothing as to the wideness of the leaching, and is therefore no objection to the ascension theory as properly understood. So also the change of vein contents of the same vein with change of country rock is precisely such a change in degree and kind as we should expect on a true ascension theory. The change is often considerable, but not so complete as would be the case were the contribution strictly local—not so complete as we actually find it in infiltration veins and amygdules. The reason is obvious. In true metalliferous veins the surface country rock is a factor, true: but only *one* of the least of many factors determining the result.

With the removal of this misconception, we believe all his objections fall to the ground; but we will also briefly notice some of these objections. Dr. Sandberger maintains that in order to make deposit, the solution must be in a state of quiescence inconsistent with the idea of water issuing in springs. But evidently the quiescence can be only relative. Some movement of the water is necessary in order to bring indefinite supply of material to the vein—unless indeed we resort t occult forces of which we know nothing. All subterranea waters are in movement—in a circulation which completes itsel only by issuance on the surface. The movement may be slo or rapid. The issuance may be, as imperceptible moistur evaporating on the surface, or may be in the form of springs o every degree of boldness. Now it is probable that a very slo movement is most favorable for mineral deposit in fissures, an that for two reasons. In the first place if the supply of wate is abundant the percentage of soluble matter may be too smal to deposit. We see illustration of this in the phenomena o the forming and filling of limestone caves. At one time whe the supply of water flowing through them was abundant, the were hollowed out by solution; but now that the water i reduced to droppings they are filling by deposit. In the secon place, rapid movement of abundant water is unfavorable fo deposit in conduits, because the cooling on approaching th surface will be too slow. The water will carry its own temperature with it instead of borrowing it wholly from the adjacen rock. For these two reasons therefore, decided springs are ap to deposit only on the surface, where, only, the cooling is sufficient and where deposit also takes place by evaporation and oxidation. In a word all subterranean waters are in movement and such movement is necessary to bring sufficient supplies of feebly soluble matters. Also, all or nearly all circulating waters terminate their journey on the surface. Whether their issuance on the surface be called springs or not is purely a question of relative abundance of the waters; but it is especially in cases of slow movement, and issuance without decided fountains, that the conduits or waterways are most apt to be filled by deposit.

Again, Dr. Sandberger thinks that metamorphic rocks derive their vein-contents from the igneous rocks in their vicinity. We on the contrary regard the igneous rocks as contributing not so much the materials as the heat necessary for their solution. There is probably no substantial difference in the chemical composition of igneous and sedimentary rocks. They are convertible into each other and are often so converted by alternate disintegrations and re-fusions.

Again, Dr. Sandberger thinks that solfataric waters at Sulphur Bank and elsewhere deposit only at the surface and in contact with the atmosphere—that these do not fill their conduits and thus form veins, and therefore we are not to look t these as examples of the process by which metalliferous vein are generated. He thinks that deposits of vein matters at Sulphur Bank is wholly the result of oxidation by contact wit air, and likens it to superficial iron ore-deposits which as i well known are due wholly to peroxidation by contact with the air. Now there is here also a partial truth in Dr. Sandberger's view, but not such as interferes with the view that metalliferous veins are formed by solfataric action. It is true, as I have shown in a previous paper,* that at Sulphur Bank the down-going acid waters formed by oxidation meet and neutralize the up-coming alkaline waters and determine abundant deposit of silica and metallic sulphides at the point of meeting, which is only about 20–30 feet from surface; but the deposit of these vein matters is not confined to this point nor determined only by this reaction, but takes place also far beyond the influence of the atmosphere. What Dr. Sandberger has himself proposed as a test of this question, viz: deep explorations beneath Sulphur Bank, has already been accomplished and the question decided against him. In the "wagon spring cut" the solfataric waters have been followed down far below the influence of oxidation and acidification to a region where the water is strongly charged with alkaline carbonates and alkaline sulphides with excess of CO_2 and H_2S and the deposit of metallic sulphides is still abundant there. But what is still more conclusive, a shaft has been sunk to a depth of 310 feet and horizontal drifts run at five different levels. These drifts have pierced the vein which is a brecciated vein very rich in sulphides of iron and mercury. That this is not an old vein through which the solfataric waters have happened to rise, redissolving some of the metallic sulphides and redepositing them at the surface by oxidation, but a veritable vein filled by solfataric action still going on and still doing its work of vein formation, is shown by the fact that the deposit of silica is here in some places so recent that the silica is still in a gelatinous condition. It is evident that Dr. Sandberger is unacquainted with the most recent investigations at Sulphur Bank. He has also wholly misconceived some even of the surface phenomena. He is apparently unaware of the fundamental distinction between the surface phenomena and the deeper phenomena—between the chemical effects of the down-going acid waters and the up-coming alkaline waters. He even speaks of the white chalky product of acid decomposition of the surface rocks as an acid clay instead of pure silica as it really is.

Again, Dr. Sandberger thinks that when solfataric waters deposit metals, these have been derived from previously existing veins traversed by the ascending waters. This may possibly indeed be true, but does not in the least affect the theory. For we probably rarely or never see any rock which has not derived its material from previous rocks by disintegration, transportation, sedimentation and reconsolidation, and yet we study the process now going on as a type of what took place in earlier times. So also it may indeed possibly, though I think not probably, be true that we rarely see metals deposited from solfataric waters which have not been derived from previously formed metalliferous veins, and yet none the less is the process in the two cases the same, and we rightly study the one to throw light on the other.

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* This Journal, vol. xxiv, p. 23, 1882.

Finally, Dr. Sandberger regards the whole phenomena of mineral vein formation as deep-seated and forever removed from direct observation; and therefore we may not hope ever to see these causes now in operation. This view it seems to me removes the subject forever from the clear light of inductive research where recent observers had hoped to place it, and remands it to the limbo of more or less probable conjecture. But we have now shown that his reasons for so doing are not conclusive, and that we may still hope to see in solfataric action the analogue of the process by which metalliferous veins were formed.

After this discussion, which we hope has served the purpose of placing the ascension-theory in clearer light, we return with increased confidence in our previously formed views, and will now proceed to apply them in the explanation of the phenomena of mineral veins.

1. *Association with metamorphism.*—If metalliferous veins are formed by solfataric action, then we at once see why they are usually associated with the evidences of volcanic activity recent or past, and especially with metamorphism of the country rock: for solfataric action is but the feeble remnant of preceding vulcanism, and metamorphism is undoubtedly produced by superheated water under heavy pressure; and the presence of alkali is also an important factor. Our view as already stated is not that igneous rocks alone supply the materials, but rather that igneous action supplies the heat necessary for solution.

2. *Absence of surface effects of solfataric action.*—If metalliferous veins were formed by solfataric action, at the time they were forming, if alkaline sulphides predominated, white chalky siliceous residue of acid decomposition (as at Sulphur Bank), or, if alkaline carbonates predominated, siliceous sinter (as at Steamboat Springs), must have existed at the then surface. But as such surface deposits are never more than 20 to 30 feet thick—they must, except in cases where the process is still going on, have long ago been entirely swept away by erosion, and therefore the deeper parts only, i. e. the true veins exposed to view. In ordinary mineral veins we have the deeper deposits revealed but the process of filling has been hitherto obscure. In solfataric springs on the other hand we have the

process revealed, but the true vein deposits have hitherto been concealed and obscured by surface-products and surface reaction. At Sulphur Bank alone, and only by means of the deep mining operations recently undertaken there, both the surface process and the deeper results in filling the conduits are clearly demonstrated. As the present condition of the theory of eruptive rocks is the result of observation not only of surface effects as shown in active volcanoes, but also of exposures by erosion of the deeper roots of extinct eruptions, so also a rational theory of genesis of metalliferous veins must be based not only on the study of the deeper parts of such veins exposed to view by erosion, but also on the study of similar phenomena still in progress in active solfataras. The light thrown on the theory of metalliferous veins by the deeper works at Sulphur Bank is precisely similar to what would be thrown on some of the obscurest questions connected with eruptions, if we could tunnel beneath a volcano, or still better beneath a great basaltic or rhyolitic overflow, and tap the conduit dykes which supplied it.

3. *Variation in vein contents.*—If mineral veins are formed by deposits in conduits from hot waters containing alkaline carbonates and alkaline sulphides, then the nature of such deposits i. e., the vein-matters, and especially whether they be metalliferous or not, will depend upon the relative proportions of these two ingredients. Alkaline carbonates are the main solvents of vein stuff, viz: silica and earthy and metallic carbonates, while alkaline sulphides are the main solvents of metallic sulphides. This is well shown in the contrast between the phenomena at Sulphur Bank and at Steamboat Springs.* At Steamboat Springs, alkaline carbonates predominate, and therefore the surface deposit is in the form of a thick cake of sinter, only stained here and there with metallic sulphides and metallic oxides; while at Sulphur Bank alkaline sulphides predominate, and therefore the deposit is rich in metallic sulphides; but there is no crust of silica formed, because the acidification of the sulphides by contact with air neutralizes the alkali, and prevents the silica from reaching the surface. In place of a deposited crust we have here a chalky siliceous residue of acid decomposition of surface rocks. Doubtless all gradations between these extremes may be found. The richness or poorness of veins in metals depends on similar differences in ascending waters of previous geological epochs. No doubt, however, the abundance and rapidity of circulation of the waters was also an important element in determining the results, for, as we have already shown, very slow circulation is favorable for deposit in conduits; abund-

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* This Journal, vol. xxiv, p. 23, 1882, and vol. xxv, p. 424, 1883.

ant waters coming up in bold springs deposit mainly at the surface.

4. *Variation of richness with depth.*—If metalliferous veins are formed by deposit of soluble matters from superheated waters, coming up from great depths, and slowly losing heat and pressure, then the level at which deposit will commence will obviously depend on the percentage of freight of soluble matters carried. It will commence whenever by loss of heat and pressure the point of saturation is reached, and will continue thereafter to the surface. On the one hand, the percentage may be so small (either because so little has been met with, or because the waters are too abundant and rapid-moving) that deposit will take place only on the surface by drying and oxidation; on the other hand, the water may be so saturated when it enters the fissure, that it will deposit equally, or nearly equally, all the way up. Between these extremes there is every gradation, and deposit may commence at any level and continue to the surface. The same is of course true of each individual substance in solution. Each will deposit when its own saturating point is reached. Applying these principles to actual vein stuffs: silica and other common veinstones being abundant everywhere, we may regard the waters as usually saturated with these, and deposit of these will usually take place equally at all depths. But metallic matters are very sparsely and irregularly distributed in the country rock, and the percentage of these in subterranean waters, depending as it does on the composition and the temperature of the water and the quantity of the metal met with in its course, must be very variable. If the water is already saturated for that temperature and pressure with these substances when it enters the fissure, then deposit will commence immediately, and continue, and the vein will be equally rich at all depths; but if the saturation is not complete, then deposit will be delayed until by loss of heat and pressure the saturating point is reached, and the vein will be richer near the surface. We have thus far supposed all the materials derived from the lowest depths only. But this is not true. Contributions may be added at every step in the upward course by tributary waters from the wall rock. Whether these waters increase or decrease deposit will depend on their percentage of metallic freight as compared with that of the main up-coming stream. On the one hand, it is more likely to diminish it, because superficial waters are less apt to contain metallic contents than deeper waters; but, on the other hand, it may sometimes increase it by cooling more quickly the ascending waters. Again, we have supposed the deposit to take place only by loss of heat and pressure. This is doubtless the most obvious and widely occurring cause; but there are also, as we have already shown, many

chemical reactions by which deposit may take place. These may occur in any part, and determine exceptional richness in that part. Taking all these causes into account, we easily see why many veins grow poorer, some hold their own, while a few grow richer, in depth.

5. *Origin of the alkaline and metallic sulphides.*—The question of the origin of the alkaline sulphides and metallic sulphides is only remotely connected with our subject. We find the alkaline sulphides in subterranean waters — such waters deposit metallic sulphides. This would seem sufficient for our purpose. But the question of origin is an interesting one, and a few words in explanation may not be out of place. Alkaline sulphides are now forming under our eyes in many places by the reducing action of organic matter on alkaline sulphates. A notable and familiar example of this is the formation of alkaline sulphides (of magnesium and sodium) in salt marshes, by the reducing action of decaying organic matters of the marshes upon the sulphates of sea water. The subsequent decomposition of the sulphides form the sulphuretted hydrogen, so easily detectable by smell. Now organic matters in solution are of almost universal occurrence in subterranean waters, and organic matter is the most powerful and universal reducing agent in nature. Organic matters, in the form of bituminous hydro-carbons, are found at Sulphur Bank, and at other cinnabar mines. It seems not improbable, therefore, that alkaline sulphides found in solfataric waters have been reduced from alkaline sulphates by this agency.

It is probable that the metallic sulphides which we find in solution in solfataric waters, and especially in mineral veins, have been formed in the same way, i. e., have been reduced from metallic sulphates by organic matters. It is probable, therefore, that we never see either alkaline or metallic sulphides which have not been reduced from the corresponding sulphates. But, on the other hand, it is certain that we never see any alkaline or metallic sulphates which have not been oxidized from corresponding sulphides. The original form was doubtless silicates. These have passed through many changes as carbonates, sulphates, sulphides, to be again changed into sulphates in perpetual cycle. So is it with everything constituting the earth crust. We certainly never see any soil that has not been rock, nor any rock that has not been soil. We never see any stratified rock that has not been igneous rock, nor, I believe, any igneous rock that has not become so by refusion of stratified rock. Thus the whole material of the earth crust has been worked over and over again many times, passing thus through a perpetual cycle of changes.

6. *Heat not always necessary.*—We have said that the solutions

depositing metalliferous vein matters have been hot. Such undoubtedly they were in most cases. Metalliferous veins are usually associated with vicinity of dykes and other evidences of igneous action. They are usually associated with tilting, folding and metamorphism of the rocks. The water cavities of vein stones have often a vacuous space, showing that the water has cooled and contracted since the veinstone was consolidated. We find deposits of vein matters, both matrix and metallic ore, now going on in hot alkaline waters, and therefore presumably they were deposited from similar waters in previous epochs. But heat is evidently not necessary in all cases. For example, (*a*) deposits of iron, as we know, often belong to an entirely different category. This metal may indeed be often found in veins, and is then probably deposited like other vein metals; but it is deposited also, and more abundantly, in beds, being leached out of rocks and soils by solutions of organic matter at ordinary temperatures, as first explained by Bischof, and afterwards more fully by Sterry Hunt. (*b*) Lead also occurs very abundantly in undisturbed regions and in unchanged, even fossiliferous, strata, mostly limestones. In the lead regions of Illinois, Iowa and Missouri, for example, it occurs in fossiliferous Paleozoic limestones, in immense deposits, partially or wholly filling irregular cavities of great extent between the strata and between the joint-blocks (gash veins). The cavities have been formed, like cavities in limestones everywhere, first by shrinkage, and then enlarged by the solvent power of water containing CO_2. The lead sometimes wholly fills the shrinkage cracks, but often these have been enlarged by solvent power of water to veritable caves. In such cases a red clay, the residue of a solution of impure limestone, is found associated with the galena in the cave.* That lead in these cases was deposited from solution is certain; that the solutions were alkaline sulphides is probable; but that the solutions were hot seems improbable. (*c*) Lastly, small cracks produced by crushing of the rocks, and small vacant spaces of any kind, such as fossil-cavities, vapor-cavities, etc., are filled by percolating waters at ordinary temperatures. This is so well known that further mention of these is unnecessary. These also may be, although they are not usually, metalliferous.

7. *Occurrence of gold.*—We have seen that hot waters containing alkaline carbonates and alkaline sulphides, coming slowly towards the surface, and losing heat and pressure, will hold in solution and gradually deposit in their courses quartz and earthy carbonates, metallic sulphides and carbonates. These are by far the commonest forms of vein

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* Whitney's "Metallic Wealth," p. 410 *et seq.* I can also confirm this from personal observation.

matters. Most other forms are accounted for by subsequent changes in these. But there are some metals which occur in a native condition, as, for example, gold, platinum, etc., nearly always; mercury and silver frequently; copper sometimes. It is well to observe that the occurrence in a native condition is common just in proportion to the feebleness of the affinities of the metal. In the case of mercury, silver and copper, it is almost certain that the original condition was sulphide, and the native condition the result of subsequent change, the nature of which may sometimes be traced. May we not then conclude that the same is probably true also of gold and platinum? We wish, therefore, to apply the foregoing theory also to the genesis of auriferous veins, for the occurrence of this metal is probably the most difficult of all to account for, by this or by any other theory.

The conditions under which gold usually occurs, viz: associated with metallic sulphides, especially iron sulphide, in quartz veins, leaves no doubt that it also was deposited from solutions. Was there any peculiar solvent differing from what usually occurs in subterranean waters depositing metals? I think not. Gold is freely soluble in a solution of free chlorine, but as this is not found in nature it is not a probable solvent of gold. Gold is also feebly soluble in salts, especially persalts of iron, probably forming with them double salts. This is a possible solvent in some cases. Again, gold in form of sulphide is decidedly soluble in alkaline sulphides, forming with them a double sulphide. But the sulphide of gold, like its oxide and salts, is extremely unstable, and on the slightest provocation gives up its sulphur and is reduced to the metallic condition. It seems to me most probable, therefore, that the gold, like and along with other metals, was in solution as a sulphide, and deposited at the same time, but that on account of its feeble affinities it gave up its sulphur to the alkali at the moment of its deposition. Auriferous quartz veins have been filled, like other veins, by deposit from hot alkaline carbonate and alkaline sulphide waters, holding in solution metallic sulphides, among which was gold sulphide. The deposit took place probably by loss of heat and pressure, the gold giving up its sulphur to the alkali, either forming alkaline persulphide, or else displacing carbonic acid at the moment of its deposition. If the gold sulphide was the only metallic sulphide present, then the gold would be found free in the pure white quartz, as is often the case in California; but if other metallic sulphides, especially iron sulphide, were present in abundance, then the gold deposited at the same time with the iron sulphide would be enclosed in the latter.

We have preferred to ascribe the deposit to loss of heat and pressure, but any one of the reactions given on p. 5 may accomplish the same result. For example, if a double sulphate o iron and gold may exist in the presence of alkaline bicarbonat and alkaline sulphide (which, however, seems more than doubtful), then we may well imagine that organic matters in solution would reduce these to sulphides, the gold returning to the metallic condition as before. Or acids of organic decomposition might neutralize the alkali and force it to deposit the metallic sulphides in solution, the gold as before returning to the metallic condition.

I have assumed that the gold is in a native state. This is by no means certain in all cases. Gold exists as telluride, and may exist as sulphide along with other sulphides. In that case the difficulty of explaining the deposit would be much less.

These views are, I believe, substantially sustained by the observations of Arthur Phillips on the re-solution and re-deposit of gold probably still going on in the deep placer gravels of California. These gravels have been, and still are, traversed by alkaline waters, certainly alkaline carbonate, and probably also alkaline sulphide. The waters were certainly at one time, if not now, hot; for the gravels are deeply covered with lava. These alkaline waters dissolve out the silica from the slate bed-rock and the slate and volcanic pebbles, leaving these as tough, soapy blue clay (putty stones), and re-deposit the silica in their course, cementing the gravels and petrifying the drift-wood.[*] On the partially petrified drift-wood, in some places the silica is still found in a gelatinous condition, showing that the process is still going on. That the same waters carried iron in solution is proved by the abundant deposit of iron sulphide on the drift-wood. That they also in some cases carried gold is shown by the fact that in a few cases the iron sulphide thus deposited, when dissolved in nitric acid, leaves gold in crystals, threads and scales, like those found in true auriferous veins.[†]

Such are the phenomena. Now the explanation. Since in this case the water is now at ordinary temperature and moving nearly horizontally, and therefore not losing heat and pressure, the simplest explanation seems to be as follows: alkaline carbonate and alkaline sulphide waters, circulating through the gravels, dissolved and redeposited the silica, cementing the gravels and petrifying the drift wood. They dissolved also both iron and drift gold found in their course, and carried them in solution as sulphides of iron and gold.. Coming in contact with decaying drift-wood, the alkali was neutralized by the acids of organic decomposition, the silica and the two metallic sulphides were all deposited, the gold giving up its sulphur at the moment of its deposition. Or if it be possibl

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[*] Old River Beds of California, this Journal, vol. xix, p. 176, 1880.

[†] Arthur Phillips, Phil. Magazine, vol. xliii, p. 401, 1872.

(which I think it is not), that iron and gold should exist as sulphates in the presence of alkaline bicarbonates and bisulphides, we may suppose these were reduced to sulphides by organic decomposition and the gold set free as before. In the deep gravels with the lava cap above, the slate bed rock beneath and solfataric waters flowing between and depositing silica and metallic sulphides in their course, we have exactly the phenomena of metalliferous-vein-formation, except that in this case the water-way is horizontal. It is in fact a horizontal vein.

8. *Different kinds of veins.*—If we assume then as probable the main principles sustained in the preceding pages, it is not difficult to account for all the different kinds of veins described in practical works. It is certain that metalliferous veins have been filled by deposits from circulating subterranean waters. These waters may have been at any temperature, but mostly hot, because these are the more powerfully solvent. They may have circulated in any direction—up-coming, down-going or horizontally moving—but the up-coming, especially in volcanic and metamorphic regions, deposit most abundantly, because they are hot, under heavy pressure and usually contain alkaline carbonates and alkaline sulphides.

a. Fissure veins.—Again: Subterranean waters deposit in their courses, i. e., in water-ways; but these subterranean water-ways are infinitely various in character. The most perfect water-ways—the highways of up-coming subterranean waters—are the great fissures made by crust-movements. The fillings of these therefore form the richest, the most reliable and the most typical of veins, i. e., veins with their vein stuff, their selvage and their walls, all quite distinct. The clay selvage which is so characteristic of a true fissure vein is evidently wall rock leached of its silica by alkaline water. It is exactly analogous to the putty stones of the deep placers, and to the tough blue clay found at Sulphur Bank in the course of the ascending solfataric waters.

b. Incipient fissures—Irregular veins.—But crust movements often produce, not clear open fissures, but only incipient fissures, i. e., slight irregular fracturing and loosening of the rock along certain vertical or highly inclined planes. Such a plane of slightly and irregularly fractured and loosened rock inevitably becomes a water-way and by deposit forms an irregular vein composed of an infinite number of small veins branching and running together again in all directions, and enclosing masses of country rock between. Many irregular veins without distinct continuous wall, like some of the cinnabar veins of California, and possibly some so-called segregative veins, come under this head.

c. Brecciated veins—Or sometimes by repeated back and forth movement, the rock along such a fractured and loosened plane may be broken into small fragments like rubble. Such a plane of shattered rock—such a fissure filled with rubble of country rock—by deposit forms a brecciated vein, i. e. a mere rubble of country rock cemented with vein matter often rich in metals Examples of such veins are not uncommon. In the Bassick mines near Silver Cliff, Colorado, the vein consists of a rubble o trachytic rock cemented together with argentiferous galena A fine specimen of this remarkable vein stone, is now in th University Museum.[*] In the carbonate mines, San Francisc district, Beaver Co., Utah, the vein which is 2 feet thick, consists of andesitic breccia with a cement of galena.[†] In th Cinnabar mine at Sulphur Bank, Lake Co., California, the vei is a rubble of shale and sandstone country rock, cemente with silica and sulphides of iron and mercury, and the proces is still going.[‡]

d. Substitution veins.—Again: if the country rock be soluble as for example limestone, then the country rock may be removed in the course of the subterranean waters, and similar o different and less soluble matters may be deposited in its place These are substitution veins. In fact the stalactitic and stalagmitic deposits in limestone caves may be regarded as subst tution veins on a large scale, the mere change in the abundanc and therefore in the degree of saturation of the waters havin determined deposit instead of solution. For as the relativ amount of suspended matters in superficial waters determine whether there be erosion or deposit along their channels; s the relative amount of soluble matters in subterranean water determines whether there be solution or deposit along thei water-ways. The lead veins of Illinois, Iowa and Missour and also of Leadville[§] come under this head. Probably als many so-called segregative veins may come under this hea They are irregular substitution veins.

e. Contact veins.—It is obvious that immediately along plane of contact between igneous and stratified rock, or betwee one igneous rock and another erupted at a different time, there will be a plane of weakness, and as such liable to be opened by crust movements. Every such plane of weakness will inevitably be found by subterranean waters and become a way of ascent. The filling of these form contact veins.

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[*] The breccia of Bassick mines is somewhat similar to that of Sulphur Ban It consists of fragments of country rock slightly rounded at the corners a edges and covered with several layers of metallic sulphides. The evidence deposit from ascending waters is complete. For an interesting account Grabill of this remarkable mine, see Trans. of Am. Institute of Mini Engineers for 1882.

[†] Becker. 1st Ann. Rep. of U. S. Geol. Surv., p. 38.

[‡] This Jour., vol. xxiv. p. 23 et seq., 1882.

[§] At Leadville the country rock is a dolomitic limestone with intercalary be of porphyritic trachyte. The metals are supposed to have been in the porphy in a finely disseminated condition, and to have been leached out and accumulat in hollowed-out channels in the limestone, in a gangue of iron, manganese a clay, partly or wholly filling the cavities. The ore was originally in the form sulphide, but some of it is changed into oxides and carbonates. Emmons, Rep. U. S. Geol. Surv. for 1881. pp. 203–231.

f. Irregular ore deposits.—Even porous strata, as gravel or sandstone may become a water-way and metals may be widely and irregularly deposited in them. Such deposits in proportion to the wideness of their distribution are poor; but in many cases the water following certain channels will deposit more richly in irregular streaks; or else the deposit is determined by the presence of organic matters, such as fossil plants or animals, and therefore richer in irregularly distributed spots. Such are probably the copper and silver-bearing Triassic sandstones of New Mexico and Utah,* and the process is now reproduced in the deep placer gravels as already explained. It is not improbable that some segregative veins may fall under this head. Even down-percolating waters may deposit metallic sulphides as shown by the occurrence of these as stalactites depending from the roofs of cavities.†

It seems unnecessary to explain still other kinds of veins or ore deposits such as gash veins, stockworks, fahl bands, etc. which are minutely described by practical writers—because once clearly apprehended the principle that subterranean waters deposit everywhere in their courses when conditions are favorable, and all these infinite varieties lose their significance.

Thus then, subterranean waters of any kind, but especially alkaline, at any temperature, but mostly hot, circulating in any direction, but mainly up-coming, and in any kind of water-way, but mainly in open fissures, by deposit form metalliferous veins. It is evident, therefore, that the form, appearance and mode of occurrence of veins must be infinitely various, but the mode of formation is substantially one. The study of these varieties may be and doubtless is, very important to the practical miner and the mining expert, but is hereby shown to be of little value to science proper, except as illustrations of the one principle.

Berkeley, Cal., March 27. 1883.

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* Cazin, Newberry etc., Report of Nacemiento Copper Mines, New Mexico.

† Schmidt, this Journal, vol. xxi, p. 502, 1881.

ART. II. — *Evolution of the American Trotting Horse;* b
FRANCIS E. NIPHER.

[Read before the St. Louis Acad. of Science, May 7, 1883.]

IN the April number of this Journal, Professor Brewer give a table of data for the number of horses capable of making o beating various speeds ranging from 2^m 30^s to 2^m 11^s, for th series of years from 1843 to 1882. The three variables deter-mine a surface, having the equation

$$N = f(s, T)$$

where N represents the number of horses capable of trotting mile in *s*-seconds or better, and T represents the year estimate from any origin in time. This surface was constructed fro the values given by Professor Brewer, and it was at onc observed that for the lower speeds 2^m 30^s and 2^m 27^s, the surface was not continuous with the other speeds. The surfac changed abruptly between the speeds 2·27 and 2·25. Thi abrupt change is perhaps explained by the fact that when 2·3 was called a fast time, less general attention was paid to breeding trotters, so that N did not increase as rapidly as now. I later years when such animals are only considered valuable a roadsters and unfit for breeding for the turf, it is probable tha an increasing number has been lost sight of, or remain undiscovered in private hands.

In plotting the values log. N and T, each of the speeds gav a straight line, the lines for the speeds 2·30 and 2·27 being represented by the equation

$$\log. N = 0{\cdot}075\,T$$

while for all the higher speeds we have, very nearly at least,

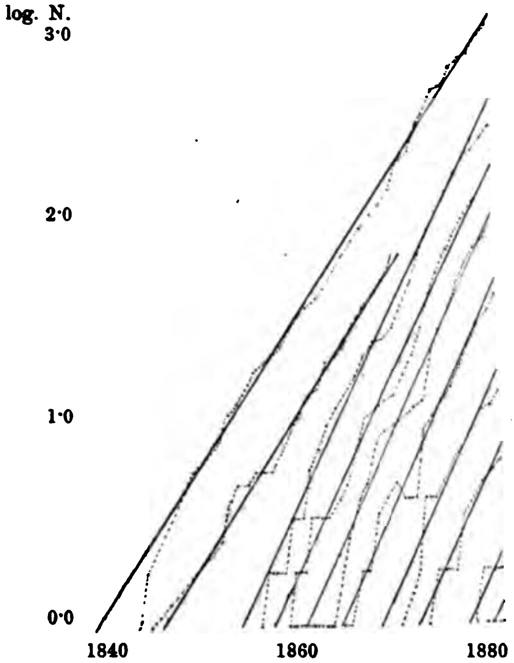
$$\log. N = 0{\cdot}10\,T$$

where for each line, T has its origin at the intersection of th line with the time axis. These lines are shown in figure 1.

It is evident by inspection that the values for 2·27 and 2·3 are incomplete, as the lines for 2·27 and 2·25 would cross at th year 1880, indicating that as many horses could make 2·25 o better as 2·27 or better. I have therefore thought it imprope to use the data for the speeds 2·30 and 2·27 in the subsequen discussion.

Referring now to the lines which represent the other an higher speeds, it will be observed that the intersection of an line with the time axis, determines the date when for tha speed, log N is zero or N is 1. In other words it gives u a calculated date when this speed may be supposed to hav had its origin. To put the matter in language having no re erence to real physical conditions, it determines the date when the amount of horseflesh capable of making this speed had

1.



increased to one horse. It is clear that this date, based as it is upon all the succeeding values through a series of years, is much more reliable than the date when some accidentally occurring trotting match revealed the fact that the horse capable of making the speed had already come.

The dates for the origin of the 2·11 and 1·13 speeds cannot yet be determined very exactly, and in the following discussion this is to be borne in mind. The same holds true for speeds originating prior to 1840, and the additional consideration that the interest in breeding such animals was much less general then than now, the conditions of evolution being therefore different, has caused the rejection of such data for the present discussion.

The following table gives the calculated dates for the origin of the various speeds, where *s* represents the time required to trot a mile. In the third column the change in speed per annum is given, being calculated in a well known manner from alternate differences in the first two columns.

s	date.	$\frac{ds}{dT}$
145	1854·0	
143	1857·4	0·57
141	1861·0	0·55
139	1864·7	0·50
137	1869·0	0·50
135	1872·6	0·43
133	1878·3	0·48
131	1881·0	

On plotting the values of $\frac{ds}{dT}$ and s, we get a somewhat ular series of points which, however, are represented fairl by the equation

$$\frac{ds}{dT} = -a + bs$$

This line is shown in figure 2. It will be observed th two points, which are farthest from the line, correspond speeds 2·11 and 2·13.

The values of the constants in equation (1) are deter by well known graphical methods to be

$$a = 1·289$$
$$b = 0·014$$

It is evident that the condition giving the limiting sp $\frac{ds}{dT} = 0$. This condition in (1) gives the maximum spe which the American trotting horse will constantly approx but never reach. This speed is a mile in 92 seconds or 1

Equation (1) can be put into the form

$$\frac{ds}{s - L} = b\,dT$$

where L is the limiting speed or $\frac{a}{b}$.

This equation admits of direct integration as follows:

$$\int_{s_0}^{s} \frac{ds}{s - L} = b \int_{T}^{T_0} dT$$

or performing the indicated operations

$$l(s - L) = l(s_0 - L) + bT_0 - bT$$

or putting the absolute term equal to A

$$l(s - L) = A - bT$$

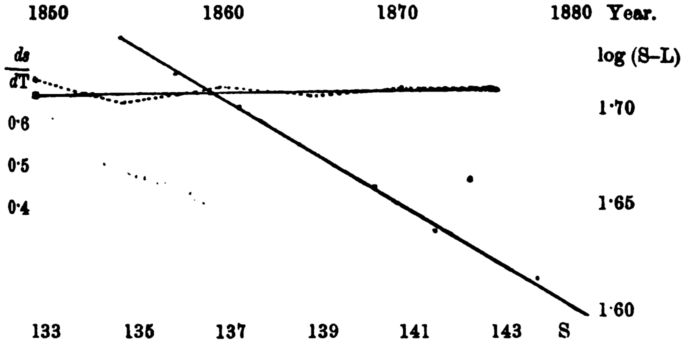
or finally for the primitive function

$$s = L + e^{A - bT}$$

where e is the Naperian base, and where A is the value of $l(s-L)$ at any arbitrary zero in time, T being estimated in years from the same zero. The following table contains the common logarithms of $s-L$ for the corresponding dates, and these two variables are also plotted in figure 2.

Year.	$s-L$	$\log(s-L)$	T	$(s-L)$ calc.	diff.
1854·0	53	1·724	− 6 0	52·7	−0·3
1857·4	51	1·706	− 2·6	50·8	−0·2
1861·0	49	1·690	+ 1·0	48·9	−0·1
1864·7	47	1·672	+ 4·7	47·0	0·0
1869·0	45	1·653	+ 9·0	44·8	−0·1
1872·6	43	1·633	+12 6	43·1	+0·1
1878·3	41	1·613	+18·3	40·5	−0·5
1881·0	39	1·591	+21·0	39·3	+0·3

2.



Estimating T from the year 1860 and taking common logarithms, we obtain, by graphical methods, from the plotted line

$$\log(s-L)=1{\cdot}694-0{\cdot}0047T \qquad (5)$$

Substituting in this equation the values of T, and re-calculating the values of $s-L$ as given in the fifth column of the table, it is seen that the greatest difference between the calculated and observed values of s, for any date of the table is half a second, which corresponds to an error of about a year in the date for a given speed. The differences in the final column show that a readjustment of the constants in eq. (5) would make the agreement better, but at present it is hardly worth the trouble, as the values would not be materially changed.

By making $s-L=1$ in (5) we have

$$1{\cdot}694-0{\cdot}0047T=0$$

or T=360 which is the number of years after 1860 when the time of the trotting horse will be reduced to within one secon of the limiting value.

It will be understood of course that it is not claimed tha the numerical values here determined are at all precise. In all probability the true value, L, is somewhat larger than 92 seconds, and may possibly be as great as 100. This value can probably be determined with considerable accuracy in the course of ten years. It is, however, quite clear that the limiting speeds of trotting and of running horses, can differ at mos by only a very few seconds.

ART. III.—*The Burning of Lignite in situ*; by CHARLES A. WHITE.

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TRAVERSING those portions of Colorado, Wyoming, Montana and Dakota which are occupied by the Laramie Group, one often observes that portions of its strata which are exposed in the bluffs and buttes have a conspicuous brick-red color. Upon close examination of at least a large part of these reddened strata it is evident that they originally bore the buff, bluish or yellowish colors of their associated strata, and that they have received their present red color from the same source that bricks do, namely, from heat. Also scattered upon the slopes and among the debris where these reddened strata exist, there are frequently to be seen masses of slag, such in appearance as results from furnace fires or from the consumption of impure coal. Much of it is plainly seen to consist of partially fused rock, and masses are common which have the appearance of true volcanic lava; to which source indeed many persons have believed them due.

Dr. Hayden made mention of these phenomena in his reports and others have done the same to some extent; but probably the fullest and best description of them that has ever been published was given by Mr. J. A. Allen in the Proceedings of the Boston Society of Natural History, volume xvi, pages 246–262.

Professor James D. Dana has also some important remarks upon the subject in his Mineralogy (1880), page 763; but my object in again calling attention to this subject is to make some suggestions as to the origin of these fires and the time within which they have taken place.

During my examination of the Laramie Group in Northeastern Montana last summer I had good opportunities for making observations upon the phenomena connected with the burning out of lignite beds there. In that portion of Montana which is traversed by the lower portion of Yellowstone River, the Laramie Group contains several distinct beds of lignite which occur at irregular intervals, ranging from near the base of the group to its summit. These lignite beds vary from mere carbonaceous seams to five or six feet or more in thickness. Practical tests that have been made of the product of many of the beds show it to be readily combustible, but it is not so durable and serviceable a fuel as could be desired. In all that region, not only in the valleys but upon the uplands, the Laramie strata have by erosion become abundantly exposed in the bluffs and bad-lands in and near the valleys, and also in the knolls and gullies upon the upland surfaces. Beds of lignite are frequently brought to view in the larger of these exposures and traces of them are also occasionally seen upon the grassy upland surfaces. Although the beds have been fired at hundreds of places it is only in a few places that those which are now exposed are seen to have so suffered near their present exposures.

In several instances, however, I was able to trace within a short distance, a bed of lignite from a point where it was unchanged and associated with yellowish and carbonaceous sandy shales and sandstones both above and beneath it, to a point where it had been burned out. At such points the place of the lignite was represented by a thin layer of unmistakable ashes containing masses of slag which had resulted from the burning of the lignite together with impurities, and the shales for several feet in thickness above and below had been reddened by the burning. Furthermore, I found at three different places in that region, a bed of lignite still burning beneath the surface. Smoke was given off through fissures in the earth and the odor of burning coal was perceptible at a considerable distance. Heat was perceptible to the touch at the surface and it was too great for the hand to bear when thrust down into the fissures which were caused by the falling in of the superincumbent strata as the lignite was consumed. The burned-out portion near by presented a like appearance with that which has just been described, and where the fire had been long extinguished.

There being no question as to the fact of the burning of these lignite beds beneath the surface, I endeavored to learn how the ignition had taken place, and when the beds began to be burned out. There seems to be only two ways in which their ignition could have been accomplished. One is by spontaneous combustion and the other by contact at exposed places of prairie fires, or fires caused by human agency. While forest fires may sometimes have resulted from lightning, it is not thought probable that a bed of lignite *in situ* could be thus ignited. I believe that in a great majority of the cases ignition has taken place spontaneously, like that which is often seen in progress in the piles of refuse coal that collect about the mouths of coal mines; and yet it is probable that in some cases the firing has been caused by the burning of grass and other vegetation upon the adjacent surface, caused by human agency.

Although, as already stated, beds of lignite are still burning at a few localities, there is evidence that of the many thousands of cases of such burnings that are known to have occurred, a large part of them are very ancient, probably more ancient than the artificial introduction of fire upon the continent.

The great erosion that the strata of the Laramie Group have everywhere suffered, even in regions where they have been little disturbed, has already been referred to. Upon the uplands of the region examined by me last summer numerous buttes and knolls occur upon the very summits of which are little patches of the heat-reddened shales, and the slopes of which are strewn with the slag of former lignite-fires. These are evidently the only remaining traces of beds of lignite that once existed at or above the horizon of the tops of these knolls. Furthermore, on the upland surfaces more or less distant from such knolls, one often meets with masses of the well-known slag which could have been transported there by no known agency, but which have doubtless settled down from the horizon where they were produced by burning lignite, as the surface was afterward lowered by erosion. These examples do not occur where erosion has been most rapid, but on the contrary they are where the minimum rate of erosion has occurred.

Such examples seem to prove conclusively the great antiquity of many of these lignite-fires, and if, as is supposed, these fires took place by spontaneous combustion as the beds of lignite became by erosion successively exposed to atmospheric influence, there is no necessity for considering the limit of their antiquity with reference to human agency in the production of fire. Indeed, taking this view of the matter there appears to be no reason why the earliest of these fires in the Laramie lignites may not have occurred as early as, if not earlier than, later Tertiary time.

ART. IV.—*On the Paramorphic Origin of the Hornblende of the Crystalline Rocks of the Northwestern States;* by R. D. IRVING.

THE crystalline rocks of the northwestern States belong to the Archæan formations and to the Keweenaw or Copper-Bearing Series. The hornblende-bearing kinds are distributed among the three general classes of (1) crystalline schists, (2) granites and syenites, and (3) greenstones—the last term being used to cover all the basic massive rocks.

Crystalline schists.—The hornblende-bearing crystalline schists are hornblende-gneisses and hornblende-schists. The hornblende-gneisses are much more abundant in this region than true mica-gneisses, although the latter not unfrequently occur. Transition forms, in which hornblende and mica are associated in all proportions, are more common, however, than kinds without any hornblende. The hornblende-schists are merely more highly lamellar and fine grained varieties than usual of the gneisses into which they grade. These schists present a groundmass of fine particles of quartz, often with some orthoclase, carrying relatively large and generally predominant hornblende grains. Transition forms between the hornblende-schists and true mica-schists (biotite-schists) also occur.

During the last three years I have examined a large number of sections of these hornblendic gneisses and schists, coming from the Flambeau* River region in northwestern Wisconsin, from the upper Wisconsin Valley † in central Wisconsin, from the Menominee River region in northeastern Wisconsin, from the Penokee region ‡ of northern Wisconsin, and from the Mississippi Valley in Minnesota. In the larger number of these sections *augite* occurs, either as a prominent accessory, or even as a dominant chief ingredient. Moreover, as was first shown by Mr. C. R. Vanhise, who did most of the work in the examination of the sections from the Wisconsin Valley, both hornblende-gneisses and hornblende-schists graduate into kinds in which augite completely replaces the hornblende. Along the Wisconsin River and several of its tributaries in southern Lincoln and western Marathon counties,§ augite-gneisses and augite-schists are prominent rocks. Even in the mica-gneisses, whenever hornblende appears as an accessory, augite is met with also.‖ The hornblende of all these schists is the more common variety, yielding greenish sections, but it shows all gradations from the fibrous forms generally regarded as characteristic of uralite, to large, non-fibrous, strongly cleaved, and deeply colored crystals.

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*Geology of Wisconsin, iv, p. 617.

† "Crystalline Rocks of the Wisconsin Valley," by R. D. Irving and C. R. Vanhise. Geology of Wisconsin, iv, pp. 627–704.

‡ Geology of Wisconsin, iii, pp. 92–99.

§ Geology of Wisconsin, iv, pp. 692–696, 702–707.

‖ Geology of Wisconsin, iv, pp. 628, 631–635, 640–642.

The first to notice augite in any of the northwestern crystalline schists was Dr. A. Wichman, who in the third volume of the Geology of Wisconsin announces its occurrence, in a variety near to sahlite, in the hornblende-schists of the Huronian of the Menominee and Marquette regions of Wisconsin and Michigan.* He also noted its occurrence in a few sections of gneiss and mica-schist. Kalkowsky, however, had already made the same observation on certain European gneisses and mica-schists.† Shortly after the appearance of Wichman's publications I studied the Flambeau River schists above mentioned and found them to be identical with those described by him from the Menominee region, save that in some sections the augite assumes a greater importance. My suspicions were then aroused that all the hornblende of these rocks might be but a paramorphic product of the augite, and when, somewhat later, I came in conjunction with my assistant Mr. Vanhise to study a suite of specimens representing a large area in the Wisconsin Valley, I was on the outlook for evidence on this point. We soon found my suspicions abundantly confirmed, for not only did we find augite occurring almost universally in the hornblendic gneisses and schists of this region, and even wholly replacing the hornblende, as above stated, but a number of sections were observed in which the augite distinctly occurs as cores to the hornblende, which is at times fibrous like uralite, but oftener is without fibrous character.

Granites and Syenites.‡—The granites of this region are in considerable part merely dependencies of the gneisses, but irregular areas of massive granite, plainly of an eruptive nature, also occur. These are sometimes mica-granites, but are also often hornblendic. Nearly all sections yet studied of the latter kind of granite show augite as well as hornblende, and frequently in the shape of cores to the hornblende, which appears not only of the uralitic and ordinary green varieties, but also in the kind known as basaltic, which yields characteristic dark-brown, very deeply absorptive sections.

A prominent instance of a granite containing hornblende is the coarse-grained rock of Big Bull Falls on the Wisconsin River. The following is quoted from a summary description of the microscopic characters of this granite, prepared, after the study of a number of thin sections, by Mr. Vanhise.*

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* Geology of Wisconsin, iii, pp. 606, 620, 628, etc.

† Tschermak's Min. Mittheilungen, 1875, p. 45.

‡ These terms, as also those applied to the massive rocks below, are used in accordance with the Rosenbusch nomenclature.

"These rocks are all very much alike in their chief characteristics. The feldspars, which are always a good deal decomposed, are in all cases the dominating constituents. The three species—orthoclase, oligoclase and microcline—are always present, the latter being usually the most abundant one. Apatite is present in all the sections, though varying greatly as to size and number of crystals. It is a constant inclusion in the feldspars, and is also often to be seen in the quartz, diallage and hornblende. The quartz, except in two or three slices, is a subordinate ingredient. Both primary and secondary quartz appear to occur. Apparently primary are the large integral grains filling interstices between the other ingredients; but in some sections a portion of the quartz is arranged within the feldspars in such a way as to suggest its secondary nature or at least its deposition after a certain amount of decomposition had affected the feldspars. The augitic ingredient in most cases is plainly diallage, and all that does not show the characteristic cleavage of diallage is much decomposed. The diallage occurs both independently, and even quite without any associated hornblende in the slice, and also in the shape of cores to uralite or hornblende individuals. It appears probable that all the amphibole is an alteration-product of the diallage. In support of this position are the following facts: The hornblende occurs both as greenish fibrous uralite, enveloping centers of diallage, and also in the shape of ordinary basaltic hornblende without included diallage, and with the characteristic color, cleavage and dichroism. Again, the latter kind of hornblende occurs as a mere border about a diallage center, and from this condition in every stage of replacement up to the complete disappearance of the diallage.† Finally, both uralite and basaltic hornblende are found, not only with a single core of diallage to each crystal, but with several or many spots of the original mineral."

True syenites, that is, massive granular rocks composed of hornblende and orthoclase as chief constituents, seem to be very rare in the Archæan of the northwest. Wichman only has described one or two instances from the Marquette region of Michigan. Among the Keweenawan eruptives, however, there is a peculiar kind of granite-like rock composed chiefly of red feldspars, including a triclinic kind which is apparently near oligoclase, as well as the predominating orthoclase, but in which augite also occurs in subordinate quantity, so that I have elsewhere described them under the name of augite-syenite.* In most sections of these rocks quartz is present, and in the mor distinctly granite-like kinds may be of a primary nature, but mos of it is plainly secondary, or at all events has been deposite after the crystallization and solidification of the rest of the rock The augite of these rocks is always in very subordinate quan tity: it is nearly always very much altered, the more ordinar forms of alteration being to a mass of nearly opaque ferri oxide. In some cases, however, the change has been to a tru hornblende or uralite.

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*Geol. of Wis., iv, p. 662.

†G. W. Hawes has described and figured a similar relation between augite and basaltic hornblende, in the Lithology of New Hampshire, pp. 57, 206; and Plate VII, fig. 1.

Greenstones.—The greenstones of the region in question ma be divided as to their mineralogical composition into the fou groups of peridotite, gabbro, diabase and diorite; and as t their geological relations, into the three groups of Laurentian, Huronian and Keweenawan greenstones.

All four of the forms of greenstone mentioned are met wit intersecting the older gneisses, much the most common bein the diabases. The peridotites, since they never have bee found to carry hornblende, do not need to be considered her Hornblende has, however, been met with in the gabbros and i every case yet studied it is plainly derived from augite or dia lage.‡ The diabases include both olivine-free and oliviniti kinds, the sections so far as examined being usually free fro hornblende, but whenever this mineral occurs it is alwa plainly altered augite. The diorites are not common, the onl sections thus far examined coming from Rib River in the wes ern part of Marathon county, Wisconsin.§ (N. W. ¼, Sec. 2 T. 29, R. 5 E.) These represent a coarse-grained rock whic may be seen macroscopically to be composed essentially of dar green lustrous hornblendes, and large-sized feldspars which ofte show striations. In a thin section, however, a large number o the hornblendes are seen to contain cores of augite or diallag often several cores in a single individual.

The greenstones or basic massive rocks of the Huronia have been studied microscopically in the Marquette and Men minee regions by Wichman.‖ I have myself studied them i the Penokee region of northern Wisconsin, and in the Thund Bay, Pigeon River region of the north shore of Lake Superio where they occur on a grand scale in what has been called by Hunt the "Animikie Group." This group has been described by Logan, Bell and others as the lower part of the Copper-bearing series, and by Hunt as much newer than the latter series, but it is plainly, as I have elsewhere tried to show,[*] the exact equivalent of the Huronian of the Penokee region, and of the south shore of Lake Superior generally. In the Huronian of the Menominee-Marquette region the greenstones include all of the four types above named, but the diabases are by far the most important, occurring not only in dike form, but also in the shape of inter-bedded flows.[†] Hornblende occurs in some of these diabases as an unimportant accessory, and although Wichman failed to show its paramorphic origin,[‡] in the few sections that I have myself seen this mineral is plainly of a secondary nature. In the diorites of this region, however, Wichman found augite crystals more or less completely changed to hornblende.[§] Greenstones are rarer in the Penokee Huronian than in that of the Menominee region, and have as yet been only imperfectly studied microscopically, most of the work upon them having been done some years since, before any suspicion of a paramorphic origin for the hornblende was entertained. So far as later investigations have progressed the indications are that the hornblende of these rocks also is secondary.

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* Third Annual Report of the Director of the U. S. Geological Survey, p. 11 also the "Copper-Bearing Rocks of Lake Superior," vol. v, Monographic series the publications of the U. S. Geol. Survey. This name is unsatisfactory, sin the rock is in the main composed of feldspars, usually more or less corroded quartz. It grades through semi-porphyritic kinds, which might perhaps be call granitic-porphyry, into true quartz-porphyry. But I am reluctant to coin a ne name.

† The term Laurentian is here used provisionally only to cover those gneis and schists which seem plainly older than the Huronian.

‡ Geol. of Wis., iv, pp. 701, 710, 711. § Geol. of Wis., iv, pp. 698, 699.

‖ Geol. of Wis., iii, pp. 618, 621–630.

The greenstones of the Animikie group are displayed in a magnificent way along the shores of Thunder Bay, and inland to the west and north from there, occurring both in dikes and in great interbedded sheets. These greenstones include several varieties of gabbro and several of diabase, which I have elsewhere described.[‖] Hornblende is not often met with in sections of these rocks, but when occurring seems always to be beyond question secondary.

The Keweenawan greenstones include several varieties of each gabbro and diabase. I have described them in detail elsewhere.[¶] Hornblende on the whole is unusual in any of these rocks, but when it occurs it is almost invariably uralite; that is, plainly secondary to augite. In the sections of one unusual variety of gabbro met with in Ashland County, Wisconsin, which I have described as hornblende-gabbro,[**] in place of the ordinary uralite, or in addition to it, there occurs a deep brown intensely absorptive basaltic hornblende. Pumpelly first described this rock in the third volume of the Geology of Wisconsin, under the name of augite-diorite,* this name being given because he regarded the hornblende as primary and the rock as intermediate between diabase and diorite. In the same volume I suggested that the hornblende was secondary and that the rock was merely an altered gabbro.† This opinion I find sustained by a re-examination of Pumpelly's sections, and by the study of a number of new sections The hornblende is found in every stage of growth from an augite crystal, with a slight border of hornblende, to a completed crystal without trace of augite.

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[*] Third Annual Report of the Director of the U. S. Geol. Survey, pp. 159–163. Also "Copper-Bearing Rocks of Lake Superior," pp. 367–386.

[†] These interbedded diabases of the Huronian have been described by Brooks and others as metamorphic, but there can I think be no question as to their eruptive origin.

[‡] Geol. of Wis., iv, p. 607.

[§] Geol. of Wis., iii, p. 628.

[‖] Third Annual Report of the Director of the U. S. Geological Survey, pp. 157-163; also, more completely in my memoir on the Copper-Bearing Rocks of Lake Superior.

[¶] op. cit.

[**] Copper-Bearing Rocks of Lake Superior, pp. 56–58.

Thus, after an examination of about a thousand thin sections representing the crystalline schists, acid eruptives and basic eruptives of a region some four hundred miles in length by 300 in width, and of three distinct geological systems, I have found no hornblende that is not either clearly, or very probably, secondary to augite.

February 1, 1883.

ART. V.—*The Bishopville and Waterville Meteorites;* by M. E. WADSWORTH.

1. *The Bishopville Meteorite.*

THE meteorite which fell at Bishopville, South Carolina, March, 1843, has been regarded as an interesting and peculiar one. Professor C. U. Shepard in 1846 (this Journal, II, ii, 380–381), described from it, under the name of *chladnite*, a mineral which he regarded as a tersilicate of magnesia, and as forming over two-thirds of the stone. The color was snow-white, rarely tinged with gray. Luster pearly to vitreous, translucent. H. 6–6·5; sp. gr. 3·116. Fuses without difficulty before the blowpipe to a white enamel. He further describes as *apatoid* some very rare, small, yellow, semi-transparent grains, having a hardness of 5·5. A third mineral which he named *iodolite* was of a pale smalt-blue color, vitreous luster, brittle. Hardness 5·5–6. Fuses easily with boiling into a blebby, colorless glass. This was found only in a small quantity.

Later, Shepard gave a fuller account of this stone, holding that it contained chladnite ninety per cent, anorthite six per cent, nickeliferous iron two per cent, and two per cent of magnetic pyrites, schreibersite, sulphur, iodolite and apatoid. The chladnite was analyzed and the results will be given below. (Ibid., 1848, II, vi, 411–414).

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* p. 36.

† p. 170.

The stone was next investigated by W. Sartorius von Waltershausen. He described it as chiefly made up of a white siliceous mineral forming a finely crystalline mass, with here and there little points showing metallic luster, also grains of magnetite and brown oxide of iron. The hardness of the white mineral was given as six, and the specific gravity as 3·039. His analysis is given below. His results indicated that the siliceous portion of the meteorite was composed of 95·011 per cent of chladnite and 4·985 per cent of labradorite. The former he found to be monoclinic and related to wollastonite in specific gravity, color, texture, hardness and crystalline form (Ann. Chem. Pharm., 1851, lxxix, 369–370). Later Professor J. Lawrence Smith stated that from some of his investigations "chladnite is likely to prove a pyroxene" (this Journal, 1855, II, xix, 163); and in time he published a further discussion giving an analysis which will be found below. As a conclusion from his results he said of chladnite: "It is identical in composition with *Enstatite* of Kenngott" (ibid., 1864, xxxviii, 225, 226). Earlier than Smith's last paper, some investigations were made upon this meteorite by Professors Carl Rammelsberg and Gustav Rose. The former held that the yellowish-brown and bluish-gray particles (the apatoid and iodolite of Shepard) arose from the oxidation of the nickeliferous iron or the alteration of the pyrrhotite.

The analysis of Rammelsberg is here given in connection with those of Shepard, Waltershausen, and Smith.

	Rammelsberg.	Smith.		Waltershausen.	Shepard.
SiO_2,	57·57	60·12	59·83	67·140	70·41
Al_2O_3,	2·72	----	----	1·478	----
Fe_2O_3,	1·25	0·30	·50	1·706	----
MgO,	34·80	39·45	39·22	27·115	28·25
CaO.	0·66	----	----	1·818	----
Na_2O,	1·14	0·74	0·74	----	1·39
K_2O,	0·70	trace	trace	----	----
Loss,	0·80	----	----	----	----
Li_2O,	----	trace	trace	----	----
H_2O,	----	----	----	0·671	----
MnO,	0·20	----	----	trace	----
Total,	99·79	100·61	100·29	99·928	100·05

Rose's examination showed that the chladnite fused before the blowpipe only on the edges to a white enamel (Abhandl. Berlin. Acad., 1863, pp. 117–122). Rammelsberg, in the continuation of his work, later declared that no feldspar was to be found in the stone (ibid., 1870, pp. 121–123).

Through the courtesy of Mr. John Cummings and the Curator of the Boston Society of Natural History, I have been permitted to make a microscopic examination of a small portion of this meteorite now deposited in the collection of that society.

The portion examined is a grayish-white mass resembling, a Shepard remarked, a grayish-white granite (albitic), with brow and black spots. Under the microscope it is seen to be composed of an entirely crystalline mass of enstatite, augite, feldspar, olivine, pyrrhotite and iron. The structure is essentiall granitic, and it appears to belong to the gabbro (norite) variet of the basalts as defined by myself in "Science" for March 9th 1883.

The enstatite is clear and transparent. It shows a longitudinal cleavage parallel to the line of extinction, and in some specimens this is crossed by a cleavage at right angles. It also has a cleavage which is often well marked and breaks the mineral into rhombic forms with angles, as approximately determined by several measurements, of 73° and 107°. The principal cleavage is parallel to the longer diagonal of these rhombs. It is this rhombic cleavage, probably, which has led observers to believe that chladnite crystallized in the monoclinic or triclinic systems.

The enstatite is found to contain many glass inclusions with polyhedral outlines, the planes being presumably, as is usual in such cases, the planes of the inclosing mineral. While many are arranged in the enstatite parallel to the cleavage planes, others are placed at every angle with those planes. The glass inclusions carry bubbles, microlites and rounded lenticular forms. The last are frequently at the end of the inclusion, and in some cases show the cherry-brown color of some chromite. This material, besides forming inclusions in the glass, is in lenticular and irregular rounded grains in the enstatite itself. It sometimes extends in a series of grains across the entire enstatite mass and at others is in isolated forms. These inclusions microscopically are seen to be composed of a center of nickeliferous iron or pyrrhotite, surrounded by a band of dark material, chromite or magnetite possibly. These ferruginous materials are in many cases surrounded by a yellowish-brown staining of iron which sometimes extends over a considerable portion of the mass and along the fissures. Along one plane in the enstatite numerous vacuum or vapor cavities were observed. The inclusions are seen to be crossed and cut by the cleavage and fissure planes of the enstatite, showing that they were of prior origin to the fissures.

The feldspar stands next in abundance to the enstatite and is in irregular masses held in its interspaces. It is water-clear and almost invisible by common transmitted light. Much o it is seen to be plagioclastic: but the twinning bands are s exceedingly fine and the polarization colors so bright it doe not as a rule show well this character, except with high powe and when the mineral is near the point of extinction. Th ldspar contains numerous yellowish-brown, dark and almost olorless inclusions, sometimes irregularly scattered but more ommonly arranged along planes like the fluid inclusions in uartz. These glass inclusions are of various dimensions and any contain a small bubble. Some microlites were also seen.

In the feldspar at one end of a section the enstatite was ound in minute crystals extending outward from a center orming stellate or rosette-like forms. The structure is like hat observed in terrestrial rocks in minerals formed from lteration or solution. This apparently might have been proluced in this case, either by the rapid crystallization of enstatite material of a liquid feldspathic mass, or by secondary alteration through water action on the rock itself. The absence of any other signs of alteration, except in the ferruginous materials, seems to negative the latter supposition. The ferruginous alteration can probably be accounted for by the absorption of moisture by this friable fissured stone since it reached the earth. The bands of inclusions were seen in several instances to extend from the feldspar through the enstatite, and in one case pass into another feldspar on the opposite side. This indicates that the cause of these inclusions was a general one for the rock mass, and not limited to any one mineral. The enstatite was found in a few cases inclosed in the feldspar.

The monoclinic pyroxene or augite is less abundant, and its determination less sure than is the case with the enstatite and feldspar. It is crossed by fissures in a very irregular manner, but shows in some cases the approximately right angled cleavage of augite. In its optical characters it resembles that mineral; but its polarization is more brilliant than that of terrestrial augite and resembles that of olivine. All the transparent minerals of the section are clearer and lighter colored than their mundane representatives and hence tend to show in polarized light, clearer and more brilliant colors. The augite is not however so water-clear as the enstatite, but has a very faint tinge of yellowish-green. The ferruginous inclusions are the same in this as in the enstatite.

The determination of the olivine is more doubtful, since it only appears in small irregular grains and masses, which hold a similar relation to the other minerals that the olivine of terrestrial gabbros usually does. From this and the fact that optically these masses are like olivine, they are referred to that mineral.

This stone in its mineralogical composition, its structure, bubble-bearing glass-inclusions, and microlites is like terrestrial eruptive rocks and it is presumable that it had a similar origin. If the common methods of lithological nomenclature were ollowed by the writer it would be proper for him to give this rock a name as a rock species, but in accordance with the pr ciples of his classification he prefers to regard it as belonging the gabbro variety (norite) of basalt; for he holds to the ess tial unity of the universe and sees no necessity of employi different names according as the rock comes from above below.

From the description of the mineral constituents of tl meteorite it would seem that regarding the presence of t feldspar, Messrs. Shepard and Waltershausen were correct wh Rammelsberg was not. This shows the inability of the abl mineralogical chemists to draw correct conclusions regardi the mineral constituents even of an unaltered rock. T trouble appears to reside with the instrument employed that is, with a defect in the method. Chladnite ought no lon to be regarded as enstatite of the purest kind as stated in m mineralogies but rather as a mineral aggregate of which enstati feldspar and augite are the principal constituents.

While these observations give an approximate solution the Bishopville meteorite puzzle of twenty-seven years star ing, it would be well if some one having larger amounts of tl meteorite at their disposal could make a chemical analysis of as a whole, and also analyze the minerals by the modern mic scopic—specific gravity—chemical method.*

2. *The Waterville Meteorite.*†

At about midnight, sometime in Sept., 1826, a meteor w seen to pass over Waterville, Maine, by Captain Josiah Cros of that town. It came from the southeast and passed in a curv line with a regular motion towards the earth. A moment aft it disappeared from sight, a report like that of a small cann was heard. A few days later Mr. Crosby picked up in a fie about one-third of a mile from the place where he stood wh he saw the meteor, a specimen which he regarded as a fra ment of it. Bearing upon the question whether it could be fragment of anything else or not, he stated that no glass worl existed in that part of the State, but that common brown eart ern-ware was formerly manufactured about one-third of a mi from the spot on which the meteorite (?) was found. "It wa a solitary stone, the soil consisting entirely of sandy loan There was no stone or accumulation of stones within two mile The specimen, when picked up, appeared to be a newly d tached mass. The grass upon which it lay was short an close to the ground, and was entirely unchanged in appearance

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* Read before the Boston Society of Natural History, April 4th, 1883; Scienc 1883, i, 314.

† Briefly mentioned in Science, 1883, i, 377.

The stone was presented by Mr. Crosby to Virgil D. Parris, and by him to Professor G. W. Keely of Waterville College (now Colby University). A portion of the specimen was given by Professor Keely to Professor C. U. Shepard, who published an account of it in this Journal (1848, II, vi, 414, 415), from which account sufficient has been taken to render this paper intelligible to those who have not access to Professor Shepard's original publication. His analysis gave the following results:

SiO_2	Al_2O_3	FeO	MgO	CaO	Total.
70·00	18·50	8·00	2·59	1·90	=100·99

It was regarded by Shepard as a doubtful meteorite, and in his later catalogues has been omitted. My attention was especially called to it from the description and analysis indicating that it belonged, if a meteorite, to a group of which only one authentic specimen is known (Igast), although several doubtful ones exist. It was then a matter of importance to ascertain whether it was a meteorite or not; and if one to ascertain its microscopic characters. On inquiry of my friend and colleague Professor C. E. Hamlin, I learned that the main mass of the specimen was presented to the cabinet of Colby University by Professor Keely in 1871. Through the kind offices of Professor Hamlin, who is now a trustee of that college, the specimen was placed in my hands for microscopic examination.

It is a small triangular cinder-like mass, cellular, laminated, and on the fresh fracture of an ash gray color. The laminated appearance is produced by a series of flattened cells surrounded by a black vitreous mass. The original surfaces are coated with a gray, red-brown and bluish-black crust formed by fusion. The formerly upper portion of the mass, when examined under a lens, is seen to be worn and polished, as siliceous rocks are apt to be when exposed to rain, while the remaining parts of the surface are found to be coated to some extent by earthy material the same as rocks are when lying in a dry sandy soil. Its cavities contain in places a fine brown matted mass formed by the fibers of growing plants, and under the microscope their vegetable character can readily be distinguished.

The specimen then when picked up by Captain Crosby could not have been a newly detached mass, but had been for a long while partially buried in the soil, and of course could not have been a portion of the meteor which he saw. It remains then to consider the very improbable supposition: is it a fragment of a meteorite which fell at some former period?

Microscopically it is seen to be a cellular glassy mass which has begun to devitrify, and presents the appearance of a slag-like body, which has been long exposed to the action of atmospheric agencies. The sections were cut across the laminations, and showed a fluidal structure parallel to it. A few quartz grains which were cracked and fissured were seen. Near the fissures numerous ferruginous globulites had been developed and the quartz showed evident signs of having been exposed to strong heat. Adjacent to the flattened, as well as some other cells, is a black and brown ferruginous material.

The sections show not the slightest trace of characters belonging to any meteorite that has yet been examined microscopically either by myself or by others, so far as can be ascertained by their published descriptions. It is apparently a slag and most probably derived from the earthern-ware manufactory at some earlier date.

No blame attaches to Mr. Crosby, for he undoubtedly acted to the best of his knowledge in making his observations and statements; and it will be noticed that his remark, that it lay upon the surface while the grass was untouched, was opposed to its meteoric origin and in accord with the results of my examination.

Cambridge, Mass., April 14th, 1883.

ART. VI.—*A Simple Method of Correcting the Weight of a Body for the Buoyancy of the Atmosphere when the Volume is unknown;* by JOSIAH PARSONS COOKE. (Contributions from the Chemical Laboratory of Harvard College.)

IT is a familiar fact that in the usual method of accurate weighing the buoyancy of the atmosphere produces a sensible effect whenever the volume of the load differs materially from that of the equipoise. But, as in all ordinary processes of chemical analysis the analyst deals solely with relative weights, the presence of a perfectly dry atmosphere does not influence his results, unless the conditions of temperature and pressure have changed between the successive weighings: and even then the effect is insignificant in most cases. Still when the volume of the vessel weighed differs from that of the weights by as little as twenty-five cubic centimeters—for example in weighing the absorption tubes used in "organic analysis"—the error caused by variations in the density of the atmosphere may be sufficient to seriously impair the accuracy of the result.

In weighing large vessels—as in determining the density of a gas—the effect of any variation of buoyancy is eliminated by the well known methods of calculating the results. But the formulas, usually given for that purpose, involve as known quantities, the volume of the vessel, the volume of the weights, the density of the air under the standard conditions, as well as the temperature and pressure at the moment of the several obervations; and since the calculations are somewhat complex, and the required data not always readily obtained, the formulas are seldom applied unless the volume of the load is quite large. Moreover in these formulas the effect of each factor can not readily be traced, and most analysts are probably not aware of the extent to which their weighings may be influenced by changes in the density of the air due to variations of temperature and pressure. In seeking to fix the weight of certain absorption-tubes (in connection with my work on the revision of the atomic weights) I have been led to a method of correcting the weights of such tubes for variations of buoyancy; which, while it does not involve the determination of any data except the temperature and tension of the air in the balance-case, and is as simple in its application as the calibration of a flask, also gives a clear conception of the effect of each variable on the weight.

It is assumed that the air of the balance-case is dry; and with one of Becker's balances I have not been able to trace any effect on the weight of a glass vessel from variations of hygrometric condition when two open dishes of sulphuric acid (three inches in diameter) were kept in the case, which has a volume of about 37 cubic decimeters. Under such conditions the only causes which sensibly modify the weight of a small glass vessel (like a closed potash bulb-tube) are the variations of temperature and pressure. The relative effect of these two variables will appear from the following considerations, which suggested the method I am to describe.

If we assume thirty inches as the standard of barometric pressure it is obvious that the variation of each tenth of an inch from this standard will determine a change of $\frac{1}{300}$th in the resultant effect of the buoyancy of the air on the load and its equipoise. Again, if we assume 27° C. as our standard of temperature—that is 300° on the so-called "absolute scale"—then according to the law of Charles the variation of each degree from this point will also cause a change of $\frac{1}{300}$th in the same resultant. In other words, counting from these standards, a variation of one degree in the centigrade thermometer indicates the same effect on the density of the atmosphere—and therefore on its buoyancy—as the change of one-tenth of an inch in the mercurial barometer. In our climate the barometer changes slowly, and its fluctuations do not ordinarily exceed one inch. On the other hand the balances in our chemical laboratories are liable to rapid variations of temperature which often exceed twenty degrees, the equivalent of two inches. Hence of the two variables the temperature is by far the more important.

If we select the two standards of temperature and tension here assumed, we can easily correct for temperature by simply adding to the observed height of the barometer (in tenths of an inch) the difference between 27° C. and the temperature observed. Of course the correction becomes negative if the temperature exceeds 27° C. Having thus eliminated the effect of temperature we can (after taking a few weighings under as great a variation of temperature and pressure as we can command) easily find the difference in weight which corresponds to a variation of one-tenth of an inch in the barometer, and we thus obtain a constant for the vessel (or other object weighed) by means of which we can rapidly reduce the weights to the standard of thirty inches barometric pressure, having previously reduced them to the standard of 27° C. for temperature. The weights, having now been corrected for buoyancy, can be compared, and although the standards may be as unusual in their association, as is one of them in its value, they are as legitimate as any others and will be found in practice more convenient.

To apply this method we simply leave the load equipoised on the balance, shifting the rider with the varying weight, and noting the corresponding temperatures and pressures, until a sufficient difference has been observed; and a difference corresponding to 20° C., or two inches of mercury, is adequate in most cases. The process corresponds to calibrating a flask, and the constant, once obtained, can be afterwards used for the same vessel, unless the weight of its contents is materially altered. The following examples will show the application of the method.

In each case the load was a closed absorption-tube of peculiarly irregular construction, but not much larger in volume than those generally used in organic analysis. We give in the accompanying tables: first, the date; secondly, the observed weight; thirdly, the temperature of the balance-case; and, fourthly, the height of the barometer at the time of weighing. These are the observed data. In the fifth column we give the reduced heights of the barometer for 27° C., and these values are obtained by simply subtracting the observed temperatures from 27°, and adding the remainders to the observed barometric heights. Below the tables we print in each case the largest weight observed over the least weight observed, and on the same lines the corresponding reduced barometric heights. Dividing, now, the difference of weight in milligrams by the difference of height in tenths of an inch we obtain the value last given, which we then called the "constant." With this constant we can readily reduce all the weights to the common standard of thirty inches, and this we do by multiplying the difference between 300 and the reduced barometric heights by this constant, and adding or subtracting the product, as the case may be, to or from the observed weights.

First Series.

No.	1883.	Weight.	C.°	H.	H. reduced.	Result.
1	May 1	87·5304	17·	304·0	314·0	87·5346
2	May 2	87·5303	17·	304·2	314·2	87·5346
3	May 2	87·5314	19·5	303·2	310·7	87·5346
4	May 3	87·5322	20·	301·0	308·0	87·5346
5	May 4	87·53205	20·5	301·9	308·4	87·5346
6	May 4	87·5320	21·	302·5	308·5	87·5346
7	May 6	87·5316	18·	300·8	309·8	87·5345
8	May 7	87·5320	19·	300·0	308·0	87·5344
9	May 8	87·5328	19·5	298·9	306·4	87·5347
10	May 9	87·53245	22·	302·2	307·2	87·5346
11	May 11	87·5333	22·	299·5	304·5	87·5346
12	May 11	87·5333	19·5	296·2	303·7	87·5344
13	May 19	87·5317	21·	303·5	309·5	87·5346
14	May 21	87·5345	23·	296·2	300·2	87·5346
15	May 22	87·5336	22·	298·0	303·0	87·5345

Greatest weight,	87·5345	Barometer,	300·2
Smallest weight,	87·5303	"	314·2
Differences,	42		14·0

Constant = 4·2 m. g. ÷ 14·0 = 0·3 m. g.

The balance here used is only sensitive under such a load to the tenth of a milligram, and hence the constancy of the results obtained is very striking. There can be no question that the mean of several weighings is accurate to the full limit of the sensibility of the instrument used. Moreover during the course of the observations it was also evident that the slight variations observed (only $\frac{3}{10}$ths of a milligram between the extreme limits) were owing to a want of exactness in the measure of the temperature of the balance-case. We used a common thermometer reading only to whole centigrade degrees; although, as afterward appeared, a difference of only $\frac{3}{10}$ths of a degree caused a difference of $\frac{1}{10}$th of a milligram in the weight, and a difference of a whole degree corresponded to the extreme difference between the observations. In the second series (made with the same tube but differently mounted) we used a standard thermometer (Geisler's make) reading to tenths of a degree, and the results are all that could be expected with the instruments employed. The observations were made, nevertheless, under the most unfavorable conditions, during exceedingly hot weather when the temperature was rapidly changing; and it was evident that the insignificant differences remaining arose from the circumstance that the thermometer was not nearly so sensitive as the air in the balance-case, following the change of temperature of the air after a considerable interval of time. It was curious to notice the slight increase of weight, caused by the radiation of the body while weighing, followed only after some time by a rise of the very sensitive thermometer employed, and this effect was obtained in weighing a vessel which displaced only about 75 cubic centimeters of air.

Second Series.

No.	1883.	Weight.	C.°	H.	H. reduced.	Result.
1	May 29	87·3447	23·5	297·6	301·1	87·3451
2	May 30	87·3432	23.2	302·0	305·8	87·3451
3	May 30	87·3437	24·5	301·8	304·3	87·3451
4	May 31	87·3444	23·8	298·8	302·0	87·3450
5	June 1	87·3429	22·8	302·4	306·6	87·3450
6	June 1	87·3432	23·75	302·4	305·65	87·3450
7	June 2	87·3419	22·6	305·2	309·6	87·3451
8	June 3	87·3420	21·95	304·5	309·55	87·3450
9	June 3	87·3427	23·15	303·8	307·6	87·3451
10	June 4	87·3441	25·0	301·1	303·1	87·3451
11	June 5	87·3443	26·0	301·4	302·4	87·3451
12	June 5	87·3446	26·3	300·6	301·3	87·3450
13	June 6	87·34435	25·55	300·75	302·2	87·3451
14	June 7	87·3452	26·7	299·0	299·3	87·3450
15	June 8	87·3464	29·4	297·9	295·5	87·3450

Greatest weight,	87·3464	Barometer,	295·5
Smallest weight,	87·3419	"	309·6
Differences,	45		14·1

Constant = 4·5 m. g. ÷ 14·1 = 0·319 m. g.*

The limits in the accuracy of the method here described are obvious; but it will be noticed that the accuracy of the method is exactly proportional to the requirement. The greater the volume of the load, and hence the greater the effect of buoyancy, the more accurately can the "constant" be found, by which the correction, required in any case, may be calculated; and, as the above examples abundantly prove, the accuracy is sufficient for the purpose in view. When the volume of the load is large it becomes necessary to measure the temperature and pressure with great precision, and to protect the balance from radiation and from all causes of rapid change of temperature. It was a great satisfaction to the writer to find that by so simple means the relative weight of glass vessels of considerable size may be determined with accuracy to the tenth of a milligram, an accuracy which is fully equal to that of the most refined processes of chemical analysis, and which enables us to attain a great degree of precision in many processes which otherwise would not be susceptible of such refinement.

From the data given, and from the known normal density of the atmosphere, it can be easily calculated that in the first series of weighings the volume of the tube and mountings exceeded that of the weights by about 75 cubic centimeters. With this difference of volume we have a variation of $\frac{3}{10}$ths of a milligram in weight for a difference of $\frac{1}{10}$th of an inch of mercury in tension, or one centigrade degree in temperature. Hence with a difference in volume of one hundred cubic centimeters we should have a variation of weight amounting to about four milligrams for every difference of one inch in the barometer, or of ten degrees in the centigrade thermometer; and these data will furnish the basis for a rude estimate of the effect in any given case. If the difference of volume amounts to 2,500 cubic centimeters, then a difference of $\frac{1}{1000}$th of an inch in the barometer, or of $\frac{1}{100}$th of a degree in the thermometer, would cause a variation of $\frac{1}{10}$th of a milligram in the weight. So also a variation in the intensity of gravity amounting to only $\frac{1}{30000}$th of the whole amount would produce a similar effect, and a sensible variation would follow any marked change in the purity of the air. Hence the balance might be used to detect exceedingly minute changes in any one of these variables, provided the others could be exactly controlled; and, although, with our better methods, these applications of the balance may be of no practical value; yet the considerations, here adduced, will serve to show how sensitive the instrument is to the slightest changes in the density of the atmosphere when loaded with vessels of large volume. The best method of controlling the weight in such cases is that adopted by Regnault in his classical work on the density of the more permanent gases. This consists in balancing the vessel, whose contents are to be weighed, with a second vessel of equal volume, the two hanging side by side in a case lined with thick felt. The balance is set over the case and the vessels suspended from the pans by means of platinum wires, which hang freely through holes in the base of the instrument.

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* In combining only the extreme weights we must obviously take care that neither of them is seriously affected by any accidental errors; and a more certain value of the constant would be obtained by combining all the observations, after well known methods. This complication however is seldom necessary, as such errors would render the final results irregular, and lead to a rediscussion of the observations.

ART. VII.—*Recent Investigations concerning the Southern boundary of the Glaciated Area of Ohio;* by G. F. WRIGHT.

A Paper read before the Boston Society of Natural History, March 7, 1883, by Professor G. Frederick Wright, of Oberlin, O.

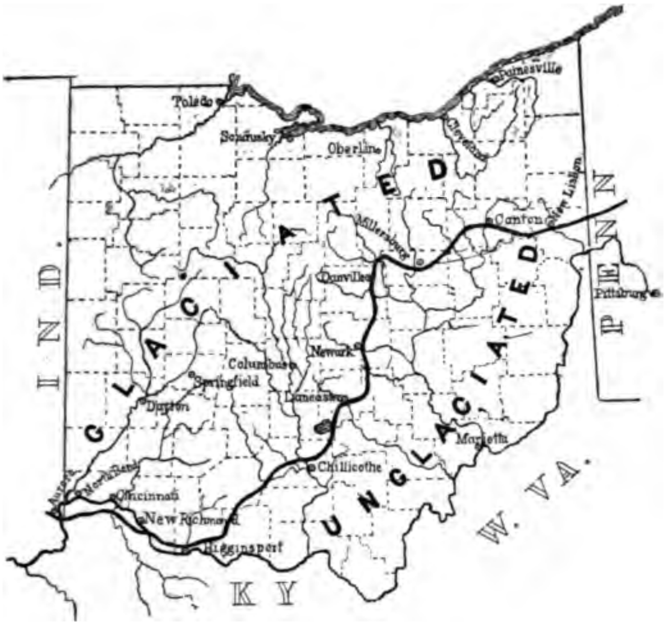
PRELIMINARY WORK.

IN the autumn of 1880, and the summer of 1881, Professor H. C. Lewis and myself, under the direction of Professor J. P. Lesley, traced the southern boundary of the glaciated area of Pennsylvania. We had also been together over a considerable part of the line marked by the terminal moraine which Professors Cook and Smock have so carefully mapped in New Jersey. I had also, from the first, been familiar with the glacial accumulations along the southern shore of New England, especially those near Wood's Holl, which Mr. Clarence King explained as belonging to the true terminal moraine of the glacial period, and which were first described by Mr. King in a communication published by me in 1877 in the Proceedings of the Boston Society of Natural History (see vol. xix, pp. 60–63), and afterwards were more fully examined by Mr. Warren Upham, and described in vol. iii, of the New Hampshire Geological Report, pp. 300–305, and in the American Journal of Science and Arts for August and September, 1879.

GENERAL REMARKS.

The accompanying map of Ohio shows the boundary line explored by me during the summer of 1882. This does not, as some may have surmised, represent merely a line which I have traversed, but a line which I have zigzagged, and along which I have fixed with certainty the glacial boundary upon nearly every mile of its course. I believe that in nearly every township I have been far enough south of the line here marked to make it sure that I was beyond the limit of glaciation. Down to this line the marks of glaciation are everywhere abundant and unmistakable; south of it the absence of glacial marks is equally striking. The average depth of the glacial deposit over the area in Ohio north of this boundary line is estimated by Mr. E. W. Claypole (see Proceedings of A. A. A. S., vol. xxx, p. 151), to be fifty-six feet. No one at all familiar with the region will be disposed to think this estimate exaggerated.

The glaciated area of Ohio consists of a rolling surface essentially like the prairies farther west, except that it was originally covered by timber. The pre-glacial channels have nearly all been buried out of sight, and it is rare that the rocks anywhere emerge above the till. The till itself contains everywhere glaciated fragments of a great variety of rocks, all of which are from the north. It is not unusual to find, many feet below the surface, granite from Canada, Corniferous limestone, and fragments of sandstone, all striated, and intimately mixed together in the paste formed by the grinding up of the Erie shales.



Large granite bowlders are thickly strewn all over the surface of this glaciated area. Some of the largest found in the State occur upon the very border of the glaciated region. A granitic bowlder found in Columbiana county, near New Lisbon, is 13×11×8 feet out of ground. Another, near Lancaster, in Fairfield county, is 18×12×6 feet out of ground. Granite bowlders from three to five feet in diameter are too numerous to mention.

Upon the southern side of the boundary line which we have indicated, the whole face of the country immediately changes its aspect; till suddenly ceases to occur; no scratched stones are to be found; granite bowlders and other transported rocks disappear, except in the valleys of the streams. Over the whole of this unglaciated area the streams flow in narrow channels cut through the horizontal strata of the coal measures and of the Waverley sandstone to a depth of from three hundred to five hundred feet, and are everywhere lined by terraces of gravel which are far above the present high-water mark. The Ohio River, from far below Cincinnati to the head waters of the Alleghany and Monongahela Rivers, a distance of more than fifteen hundred miles, occupies a narrow pre-glacial valley, and was the great distributer of the drift brought into it by the streams from the north which all along emerged during the Glacial period from the ice-front, and which in some places approached to within a few miles of the river. Upon the highlands in this unglaciated region the soil is shallow, and consists of the remnants of the rocks in place which have been disintegrated by sub-aerial agencies. The contrast between the glaciated and the unglaciated areas of Ohio appears upon the pages of the Annual Crop Report. According to the Report for September, 1882, the average production of wheat per acre in the glaciated area, reckoned by counties, is in many cases twice as great as in the unglaciated. The average production per acre in the whole glaciated area is about fourteen bushels, and in the unglaciated, nine bushels.

The southern margin of the glaciated area of Ohio is not everywhere marked by such a relative excess of accumulation of glaciated material as is found through Cape Cod, on the Elizabeth Islands and Long Island, and at various places in New Jersey and Pennsylvania. The limit, however, everywhere is very sharply defined, and at various places, soon to be mentioned, in Stark, Holmes, Fairfield and Ross counties, the marginal deposition is on a scale equal to anything which can be found on the south shore of New England.

The glacial limit enters Ohio from the east in Columbiana county, twelve miles north of the Ohio River, and continues nearly west to the middle of Stark county where it turns more to the south, crossing the northern part of Holmes county, to the northeast corner of Knox, where it turns at right angles to the south, running through the eastern part of Knox and Licking counties, the western part of Perry, turning here so as to pass through Lancaster in Fairfield county; touching the western edge of Hocking and entering Ross at Adelphi, in the northeast corner. Here it turns to the west, crossing the Scioto Valley a few miles north of Chillicothe, and emerging from the county at its southwest corner, proceeding thence through the southeastern corner of Highland, the northwestern of Adams, reaching the Ohio River in the southern part of Brown county, near Higginsport. Cincinnati was completely enveloped by ice during the Glacial period, and extensive glacial deposits exist in the northern part of Campbell and Boone counties, Ky., and near Aurora, in Dearborn county, Ind.

MORE DETAILED ACCOUNT.

Through Columbiana county as in the adjoining counties of Pennsylvania, south of the heavy deposits of till there is a fringe from one to three miles wide. Over this margin there are scattered evidences of glacial action, consisting of granite bowlders and patches of till here and there upon the highlands, at an elevation of from three hundred to five hundred feet above the Ohio River. North of this fringe the till is continuous and everywhere of great depth. At Palestine, on the eastern edge of the county, and at New Alexandria, near the western side, wells are reported in the till fifty feet deep. New Alexandria is upon the highest land in that part of the country, and the glacial deposits are marked in moderate degree by the knobs and kettle-holes characteristic of the moraine upon the south shore of New England. A mile or two west of Canton, in Stark county, the accumulations of glaciated material are upon a scale equal to anything upon Cape Cod. The northern part of Holmes county is covered with till which is everywhere of great depth, and in numerous places near the margin displays, though in a moderate degree, the familiar inequalities of the New England moraine. After the southern deflection in Knox county, the glaciated region is entered near Danville, from the east, on the Columbus, Mount Vernon, and Akron Railroad, through a cut in till a quarter of a mile long, and from thirty to forty feet in depth. At the old village of Danville near by, upon a neighboring hill, wells are reported as descending more than a hundred feet before reaching the bottom of the till. Through Licking county, both north and south of Newark, the depth of the glacial envelope is great up to a short distance of its eastern edge. At the old reservoir in Perry county, the distinct features of a moraine come out. The hill upon which Thornville is built is a mass of glaciated material in which wells descend from thirty to fifty feet without striking rock. This is upon the highest land of the vicinity. The reservoir itself seems to be in a great kettle-hole or moraine basin. All through Fairfield county, the glacial accumulation is of a great depth down to a very short distance of its margin. But perhaps the most remarkable of all the portions of this line in Ohio is that running from Adelphi, in the northeast corner of Ross county, to the Scioto River. The accumulation at Adelphi is more than two hundred feet, and continues at this height for many miles westward. Riding along upon its uneven summit, one finds the surface strewn with granite bowlders, and sees stretching off to the northwest the magnificent and fertile plains of Pickaway county, while close to the south of him, yet separated by a distinct interval, are the cliffs of Waverley sandstone, rising two hundred or three hundred feet higher, which here and onward to the south pretty closely approach the boundary of the glaciated region. Through the southeastern corner of Highland county and the northwestern of Adams, the terminal accumulation is less marked than in Ross county; still the boundary of the glaciated region is easily determined. It approaches the river in the vicinity of Ripley and Higginsport in Brown county, and crosses it from Clermont county, so as to enter Kentucky a half mile north of the line between Campbell and Pendleton counties. Cincinnati was, as I have said, covered with ice during a portion of the Glacial period. There is an undoubted deposit of till, at the railroad station at Walnut Hills, nearly 400 feet above the river. At North Bend the tunnel of the Indianapolis, Cincinnati and LaFayette Railroad, leading from the Ohio to the Miami, is through an accumulation of till which rises 200 feet above the river.

But the most interesting fact of all is, that the ice extended across the Ohio River into Campbell, Kenton, and Boone counties, Ky., leaving granite bowlders and deposits of till upon the hilltops more than five hundred feet above the river. The glacial boundary first crosses the Ohio River twenty-five miles above Cincinnati, entering Kentucky near the southwestern corner of Campbell county, nearly opposite Pt. Pleasant, in Clermont county, O. Till, containing granite bowlders and scratched pebbles, covers the hills in the vicinity of Carthage, Campbell county, and continues, to a greater or less extent, south along the ridge road as far as Flag's Spring. Here all signs of glaciation suddenly disappear. At Flag's Spring occurs an extensive deposit of water-worn pebbles which have been cemented together by lime. The pebbles are themselves mostly of lime. The deposit is in a valley tributary to Twelve Mile Creek (which runs to the north), and rises from twenty to thirty feet above the present valley. It resembles in nearly every respect the post-glacial conglomerate known as "Split Rock," at the mouth of Woolper Creek, about twenty-five miles below Cincinnati, where the glacial boundary recrosses the Ohio, and enters Indiana near Aurora. Whether there are granite pebbles in the conglomerate at Flag's Spring I am unable to say, owing to the haste with which I was compelled to examine it. But at Woolper Creek, granitic pebbles in small quantity form a constituent element of the conglomerate. One was observed which is two feet in diameter. The limestone pebbles in this conglomerate are frequently three or four feet in diameter. As pointed out forty years ago by Prof. Locke, and noticed later by Dr. Sutton, this conglomerate at Woolper Creek is not confined to the immediate vicinity of the Ohio; though there it rises more than one hundred feet above low water mark. The conglomerate is conspicuously developed on the summit of the hills for three or four miles southeast, and four or five hundred feet above the river, and here, as at Flag's Spring, on the other side of Cincinnati, the formation marks the true glacial boundary. It would seem, however, that the ice nowhere extended into Kentucky more than four or five miles from the river. Near Burlington, in Boone county, on one of the tributaries to Gunpowder Creek, which flows to the south, and whose source is between five hundred and six hundred feet above the river, there is a noticeable collection of granitic bowlders marking the southern extent of the ice. Fifteen or twenty of these, from one to three feet in diameter, were counted in a small space. Three or four of these were composed of a metamorphic conglomerate containing jasper pebbles peculiar to the eastern shore of Lake Superior.

Prof. Lewis supposes (see Journal of Franklin Institute, April, 1883) that near Olean in New York, where the ice extended for a short distance across the Alleghany River, a sub-glacial channel was kept open. It would scarcely seem possible that this was the case at Cincinnati; for the trough of the Ohio is considerably wider than that of the Upper Alleghany, and not far from fifty miles of the Ohio Valley, bordering Campbell, Kenton, and Boone counties, Ky., must have been covered by glacial ice. Probably, for a short time, the ice at Cincinnati formed an obstruction to the channel. But what was the course of its overflow I am not prepared to say. The obstruction must have been at least five hundred or six hundred feet in depth, this being the height of the watershed between the Licking River in Kentucky and the Ohio River on either side. Such an obstruction would set back the water of the Ohio far up into the valleys of the Alleghany and the Monongahela, submerging the site of Pittsburg three hundred feet. (The low water mark at Cincinnati is 441 feet above the sea, that of Pittsburg 715 feet.) It remains to be seen how much light this may shed upon the terraces which mark the Ohio and its tributaries in Western Pennsylvania.

TERRACES.

An important point of observation has been the height and character of the terraces marking the streams where they emerge from the glaciated area. I will content myself with giving some of the most important measurements and a few facts connected with each stream mentioned. In general, the streams in their course through the glaciated area have much more gentle current, and much less distinct valleys, than in the unglaciated area, and the point where a stream emerges from a glaciated area is pretty sure to be marked by terraces of exceptional height, and exceptionally coarse in their composition.

1. *Big Beaver Creek*, in Pennsylvania, emerges from the glaciated region at Chewtown, in Lawrence county. Here, the terrace is, according to White, 160 feet above the level of the stream, and is strewn with granitic boulders. At the junction of this stream with the Ohio, fifteen or twenty miles below, the terraces are remarkable and instructive. The highest is from 280 to 300 feet above the Ohio River. The towns of Beaver and Rochester are built upon a lower terrace, which is 120 feet above the level of the Ohio. The composition of that part of this terrace occupying the angle upon which Beaver City is built, and down stream from the junction, differs remarkably from that upon which Rochester is built, on the angle up stream from the junction. In the angle below the junction, the terrace is composed of rounded pebbles of quartzite, gneiss and granite, many of them even near the surface being a foot or two in diameter. In the angle above the junction, granitic pebbles are exceedingly rare, and pebbles of any kind more than two or three inches in diameter are scarce. The explanation is evident. To use Prof. Dana's term when speaking of the Connecticut River (see this Journal, Dec., 1881 p. 466), the Ohio is the great *distributer*, and the Beaver is here the principal *contributor*, of drift material. The Ohio does not itself have direct access to bowlders which have been transported by glacial ice. The drainage basin of the Beaver is, however, strewn with glacial *débris;* and, as the granitic bowlders of that area were borne along by the swollen torrents at the close of the glacial period, they would naturally, on reaching the onward, but less rapid current of the broader Ohio be deposited at the junction of the streams, and in the angle which was down stream.

Passing now in order from east to west, the more important terraces of the various streams of Ohio as they emerge from the glaciated area, are as follows: the measurements unless otherwise indicated, were made with a hand level, and in most cases the uncertainty can scarcely be more than a few inches.

2. *The Middle Fork of Little Beaver* emerges from the glaciated area at New Lisbon. The terrace here shows no distinct stratification: pebbles from 10 to 15 inches in diameter are numerous; the height above the river at railroad station is 36 feet; and it extends, with some interruptions, five miles down the river to near Elkton, but at a diminished height. These terraces are extensively mined for kidney iron ore. Granitic pebbles are abundant. At Teegarden, five miles above, the channel is narrower; the terrace, 48 feet; material, very coarse, some of the pebbles being four feet in diameter.

3. *East Branch of Sandy Creek.*—This is a branch of the Tuscarawas, which empties into the Muskingum. At East Roches-

ter, in the southwest corner of Columbiana county, occurs an extensive kame-like deposit, filling the valley which is here about one half mile wide, granitic pebbles from three to six inches in diameter are abundant. The valley runs nearly east and west. The kame, however, which is thirty feet in height, with a slope of 30°, crosses the valley at right angles, abutting against the hills upon the south. A small fork of the creek which comes in here from the north, probably explains the peculiarities of the accumulation. Following down the Big Sandy, for six miles, to Minerva, we find the accumulations of gravel gradually diminishing, both in amount and in coarseness.

4. *The Nimishillen* is likewise a branch of the Tuscarawas, and emerges from the glaciated area at Canton, Stark county. The gravel deposits in this vicinity are enormous. On the east side of the city the terrace is forty-one feet above the flood plain. All the pebbles are well rounded. The majority are granitic, but many are of local material. It is not uncommon to find them sixteen inches in diameter.

On the west side of the city the terrace rises in successive stages to more than eighty feet, and the surface grows more and more uneven, until about two miles back from the creek, it emerges into an enormous kame running nearly north and south and known as "Buck's Ridge." This ridge is identical in its structure with the kames of New England. Where it is intersected by the Fort Wayne and Chicago R. R., the kame rises eighty-five feet above the plain, is coarsely stratified, contains many granitic pebbles, one of which, 21½ feet higher than the railroad, measured 55×46×18 inches. There were large spaces in which no stratification appeared. Pebbles upon the summit from two to five inches in diameter are numerous. The section exposed shows a base of 570 feet, with an altitude of 85 feet. The slope upon the east side, towards Canton, varies from 18° to 25°, on the west side it is a little more gentle. An extensive sandy plain full of gentle swells and ridges stretches to the westward, while the space toward Canton is occupied by the more nearly level terrace. 150 yards north of the section just described begins a series of enormous dry kettle-holes, whose rims are upon a level with the top of the kame. One measured 300×200 feet, was 40 feet deep, with sides sloping inward 24°. A granitic bowlder in one of these measured 51 × 25 × 31 inches. This series of kettle-holes stretches northward, and connects with a line of glacial lakes extending to Akron, which occupies a pass on the watershed between the valley of the Cuyahoga and that of the Tuscarawas.

South of Canton one mile and a half, and just below the junction of the two branches of the Nimishillen, there are two well-marked terraces, the first of which is much the broader, and is but thirty-eight feet above the stream. The upper terrace upon the east side is seventy-four feet above the stream, and on the west side seventy-nine feet. The pebbles in the upper terrace are a mixture of granite and local rock, many of them a foot in diameter; one granite pebble measured more than two feet.

5. *The Tuscarawas River* emerges from the glaciated region two miles above Bolivar, on the boundary between Stark and Tuscarawas counties. Here upon the north side of the river (which makes an immense ox-bow opening to the south), is an enormous kame-like accumulation, very coarse where exposed near the summit, and running for a mile or more parallel with the river (nearly east and west), and bearing upon its back numerous granitic bowlders. The terrace from which this kame rises is thirty-six feet above the river, and the kame itself rises 154 feet. The ox-bow is occupied by a gravel deposit whose surface is fifty-one feet above the river. From this point down, the stream occupies a narrower valley, with diminishing terraces, but at Zoar, five miles below, wells sunk thirty feet are still in gravel. Above the ox-bow, and upon the west side of the river, a terrace sixty-one feet in height continues for a mile or more without change.

6. *Sugar Creek*, another tributary of the Tuscarawas, emerges from the glaciated area at Beech City, a few miles west of Bolivar. Extensive kame-like deposits mark the course of the railroad for several miles north of this point. One and a half miles below Beech City, towards Deardoff's Mills, the accumulations of gravel in the valley are immense. The valley is here about one mile wide. The gravel is frequently thrown up in hummocks and ridges enclosing kettle-holes, the summits of which are from twenty to thirty feet above the general level. Below this point, as seen from the cars, the gravel deposits seem gradually to diminish and become of finer material. Below New Comerstown much of the way the valley is bordered with a terrace of fine gravel from fifteen to twenty feet high. At Coshocton, where the Killbuck and Tuscarawas rivers unite, the terrace is noteworthy for its extent, but there does not seem to be much increase in its height, while the pebbles scarcely ever exceed two or three inches. At Dresden Junction I had opportunity to examine more carefully. The valley is about one half mile wide. Pebbles from one to three inches in diameter are abundant, and are largely granitic. At Zanesville the valley seems narrower, but there is no perceptible increase of terrace. A glance at the map will show that from Zoar station to Zanesville the river is nearly parallel with the glacial boundary, and from fifteen to twenty miles
ant, and is constantly receiving tributaries whose headwa-
re in the glaciated region.

7. *The Killbuck* unites with the Tuscarawas at Coshocton to form the Muskingum. It emerges from the glaciated region at Millersburg. Below this point it occupies a narrow valley from four hundred to five hundred feet below the tops of the plateau. A mile and a half below Millersburg, upon the western side of the stream, the terrace which is about a quarter of a mile wide displays numerous kame-like ridges and knolls, the summits of which are 102 feet above the flood plain. This terrace gradually merges into the till which covers the hills to the west. Two miles farther south the terrace is composed of finer material, is level topped,' and but seventy-one feet above the flood plain. Three miles farther south, at Oxford, there is a terrace upon the east side of the stream, extending across the open ends of an ox-bow formed by the stream, which is seventy-six feet above the flood plain. Not far from here Shimplins Run, a small stream coming from the glaciated region west, joins the Killbuck. Near its mouth the terrace containing some granitic pebbles, is sixty-one feet above the flood plain; but it gradually diminishes in height towards the sources of the streams. Above Millersburg there is no terrace for a mile; but at Holmesville, five miles above, where three or four streams meet, there is a gravel deposit two miles or more in diameter about twenty-five feet above the flood plain. On the east side of this plain, near the junction of Martin's Creek and the Killbuck, there is a kame-like accumulation extending one-eighth of a mile northwest by southeast. The surface is very much broken, displaying many kettle-holes. A railroad cutting through it shows some scratched stones in the material, and exposes a depth of sixty-one feet. A little to the north it rises forty feet higher.

8. *The Mohican*, the principal tributary of the Killbuck, is formed by the junction of Jerome and Black Forks, and issues from the glaciated region near the northeast corner of Knox county, not far from the point where the glacial boundary turns so sharply to the south. Jerome Fork is bordered for some distance above the junction by a terrace which rises 107 feet above the river, and is composed largely of coarse granitic pebbles. This terrace also extends for a half mile below the junction. At Gann station, eight miles below, there are extensive deposits of rather fine gravel.

9. *The Licking River* emerges from the glaciated region at Newark. Three forks unite here to form the main stream, and as usual in such places there is an extensive gravel plain stretching inward. This plain is about twenty feet above the river. The city is built upon it, and it was a favorite resort for the mound builders. For a mile and a half below the city, the river as it enters the narrower channel is bordered on the north

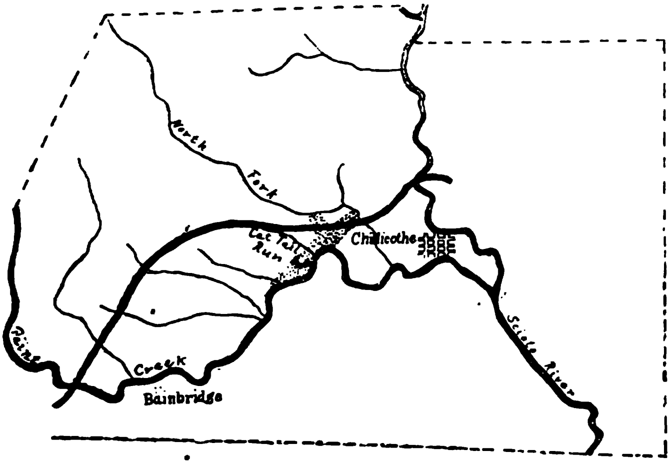
by a terrace, which, at the cemetery, is 108 feet above the river. The terrace upon the south bank opposite the city is but sixty feet.

10. *Jonathan Creek* is a tributary of the Muskingum, rising in Thorn township, Perry county, near the eastern end of the Licking Summit reservoir. The valley in which it runs afforded an important outlet to the glaciated region. It is about a mile wide, and kame-like ridges of gravel, from fifteen to twenty feet in height, extend for a mile or more down the valley, keeping near the middle. The ridges are here flanked and partly covered by extensive deposits of muck. Near the reservoir the kames are very distinct, and much higher. Following the line of the Baltimore and Ohio railway to the southeast, low terraces are continuous, until turning to the south, up another creek, whose headwaters are near Somerset, and outside of the glaciated region where terraces and all other signs of drift are altogether absent.

11. *The Hocking River* issues from the glaciated area at Lancaster. I did not examine the present valley of the stream below Lancaster, but the Cincinnati and Muskingum railway occupies the natural glacial outlet (now nearly dry) for several miles east. This is in the north part of Berne township, and just north of a continuous escarpment of Waverley sandstone, which rises abruptly 250 or 300 feet. Extensive gravel deposits extend for an indefinite distance along the course of this outlet. Gravel is specially abundant where small tributaries have come in from the glaciated region just to the north. At the mouth of Raccoon Creek, near Berne station, gravel deposits rise in hills sixty feet.

12. *The Scioto River* occupies a much broader valley than any of the preceding, and is much the largest stream issuing from the glaciated region before reaching the Ohio. The glacial boundary crosses it a few miles above Chillicothe, and the terraces from Circleville down are worthy of close study; but I must confine myself to the gravel deposits near the glacial boundary. In the southern part of section 29, Green township, three miles east of the river, there are enormous kames rising (according to barometer) 150 feet above the general level, and running in a north and south direction. The material of this kame is rather fine, and consists principally of limestone pebbles. Above this point the plain is very broad and extensive; below this, the valley contracts to a width not over two miles, and is bounded upon either side by precipitous sandstone hills, which rise 500 or 600 feet above the valley. On going from these kames to the river, westward, three broad parallel ridges are encountered, each one in order toward the river extending farther south. The second one of these rises abruptly to a

height of 48 feet above the flood plain. Kame-like ridges also appear upon the west side of the river, near the southeastern corner of Union township.



18. *Paint Creek.*—Professor Orton (Second Geological Survey of Ohio, vol. ii, pp. 653–655), briefly describes one of the most interesting results of glacial action, occurring upon Paint Creek, about 5 miles west of Chillicothe (see map). Paint Creek enters Ross county near its southwestern corner, and up to the point mentioned, flows while in the county about 40° north of east, occupying a valley about half a mile wide, which is bounded by precipitous hills 500 feet high. The natural continuation of this valley to the northeast intersects that occupied by the North Fork (running 20° S.E.) about two miles beyond the point we are considering. It is evident from the whole configuration that such was the preglacial course of the creek, but this extension is now filled with immense deposits of drift, and the main creek disappears in a narrow gorge running to the southward, cut through precipitous cliffs. This postglacial gorge is about three miles long, 300 feet in depth, and not over 200 feet wide at the bottom. Immense kames stretch across the old valley from the North Fork, especially near Cat Tail Run. Some of them are at least 200 feet above the present flood plain of the creek. Near the summit of these kames pebbles two feet through are interstratified with coarse sand, and granitic bowlders three feet in diameter rest upon their summits. A few rods above the junction of Cat Tail Run is a gravel deposit facing Paint Creek and 140 feet high, enclosing immense kettle-holes. For four or five miles up Paint Creek there continues to be a terrace from 100

to 150 feet above the stream. It is pretty clear that the ice d not extend into this portion of the valley; but it certain surmounted the hills to the north, and all the small strea coming down from them must have been gorged with wat from the melting ice. At Bainbridge, twelve miles southwe on the creek the terrace is twenty feet above the flood plai and contains great number of pebbles four or five inch through, and occasionally a granite bowlder four feet through

14. *The Ohio River* does not have a continuous terrace. Th towns of Ripley, Higginsport and New Richmond, above Ci cinnati, are built upon gravel deposits that are about sixty fe above low-water mark. These towns were all flooded in Jan ary last. The upper portion of the old part of Cincinnati i however, upon a glacial terrace that is about sixty feet highe The junction of the Miami with the Ohio below Cincinnati, characterized by gravel deposits of enormous extent. The va ley is here from three to four miles wide, and above Auror near Lawrenceburg, is bordered upon the west by a terrace who surface is seventy-eight feet above the flood plain of the rive upon which Lawrenceburg is built, and 112* feet above lo water mark. The terrace is here from one-fourth to thre fourths of a mile in width. The gravel deposit upon the Ke tucky side of the river opposite Lawrenceburg is of equ height, but much narrower. Four miles below, this grav deposit, upon the Kentucky side, is cemented together b infiltrations of lime, and constitutes what is called the "Spl Rock" formation. Two miles above Bellevue, and about t same distance below "Split Rock," occur some kame-like ridg upon the Kentucky side, whose summits are more than 1 feet above low-water mark.

CONCLUSION.

It is not best at present to speculate too freely upon the facts. Each of these tributaries to the Ohio should be car fully studied through its entire course, and much remains t be done upon the Ohio itself. Enough, however, appears t give new interest to the whole question of the glacial perio and to raise the hope that the amount of erosion afforded b the streams of the interior will afford some additional eviden concerning the date of the close of the glacial period in Ame ica. The full report of my last summer's work, with more d tailed maps, will be published by the Cleveland Historic Society, whose friends have borne my expense in the field.

ART. VIII.—*On the Variation of the Specific Heat of Water*; by G. A. LIEBIG, Student of Physics in the Johns Hopkins University.

UNTIL the year 1877 it was generally believed by physicists that the specific heat of water increased regularly from 0° to 100°. During that year, however, Professor Rowland proved by his researches on the mechanical equivalent of heat* and by separate experiments, using the method of mixture, that the specific heat of water did not increase regularly with increase of temperature, but decreased from 0° to a minimum at about 30°. In 1882, F. Neesen,† of Berlin, investigated the subject, deducing the variation of the specific heat of water from the specific heat of platinum, which he had previously determined; and although his results are not quantitatively the same as Rowland's they are so qualitatively, i. e. they show that the specific heat of water decreases from 0° to a minimum somewhere near 30°. Owing to the general interest and importance of these conclusions, I have, at the instance of Professor Rowland, undertaken during the last few months a new series of experiments. The apparatus used was that devised and employed by him in his researches, and the method of procedure was also as nearly as possible identical with his.

The two thermometers (Nos. 108,947 and 108,954) necessary for the work were made by Hicks, and graduated in millimeters. They were carefully calibrated and compared with the University Standards,‡ after which a table of temperatures was constructed for each. They were found to agree very closely with the air thermometer, verifying the conclusions reached in the article before referred to, that thermometers made of English glass are characterized by this property.

Some idea of the form of the apparatus used can be had from an inspection of the accompanying figure (taken from Professor Rowland's paper). It consisted of three essential parts; a reservoir A which was filled with broken ice, or warm water according as the experiment was to be made at low or high temperatures, connecting by means of a three-way stopcock with an interior vessel B and with the vulcanite spout C; the calorimeter E, into an orifice in which the vulcanite spout fitted closely; and a water jacket M entirely surrounding the calorimeter. The reservoir and spout—which was very short—were wrapped with a cloth to obviate change of temperature, and the calorimeter was held in place by three vulcanite st to prevent conduction to the jacket. Although when exp menting at low temperatures the vessel B was useless, stil was often kept in place and the water from the melting allowed to flow through it before entering the calorimeter. no case did the temperature of the water from the ice, whe

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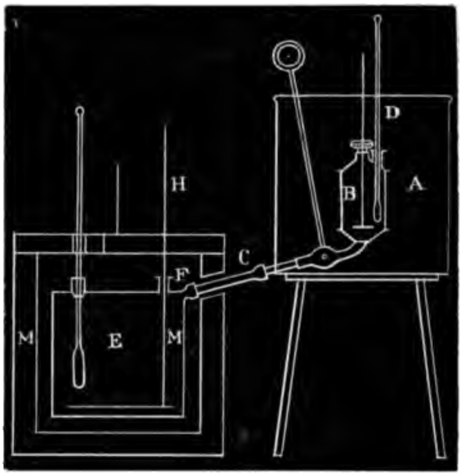
* Mech. Equiv. of Heat, Proc. Am. Academy Sciences, 1880.

† Ann. Physik u. Chemie, neue Folge, vol. xviii, 1883.

‡ These—Nos. 6,163 and 7,832—were made by Baudin, and had frequently been compared with the air thermometer. No. 6,163 was used by Professor Rowland in his before-mentioned research.



the vessel B, differ appreciably from zero. The opening i the stop-cock was always covered with fine wire gauze to vent particles of ice from being carried into the calorim with the flowing water. The calorimeter was made of very copper and lightly plated with nickel: three orifices on the permitted the insertion of the vulcanite spout and the thermometers. The weight of the calorimeter was 388·5 gr and its calorific capacity 35·4 grams. During the course of investigation, the stirrer H having been broken, it was ne sary, in order to repair it, to unsolder and afterwards reso the top of the calorimeter. In this way the weight of calorimeter increased four grams. But as the increase was entirely to solder, nothing else having been removed or ad the new capacity was readily determined. Thus, assuming der to be composed of equal parts of tin and lead, its spe heat between 0° and 40° would be about ·043, therefore

$$\cdot043 \times 4 = \cdot172$$

or ·2 must be added to the former value of the capacity, m ing 35·6 and the weight 392·5 grams.

The method of experimenting was as follows: the vesse having been filled with broken ice, the thermometers were mersed therein and after about ten or twelve minutes their readings taken. The calorimeter was, in the mean time, filled about two-thirds with water, weighed and placed in position within the jacket; one thermometer was then removed from the ice and immersed in the water in it. After a sufficient lapse of time, readings were taken every minute for six or seven minutes, to determine the time correction for radiation. The stop-cock was then turned, water flowed from A into the calorimeter, and the other thermometer having been placed in the latter, the flow of water was discontinued, and readings of both thermometers taken every minute as before. In this way the final temperature of the mixture was given as the mean of the readings of the two thermometers, one of which approached it from above, the other from below. The mean must therefore have been very nearly correct, even if thermometers, as is supposed, lag behind the true temperature. Corrections for the calorific capacity of the thermometers (equal to about 1·1 gram for each) and for protruding stem were always applied, as well as a time correction for radiation. The temperature of the jacket was always noted, but, because it never differed more than one or two degrees from that of the surrounding air, and owing to the before-mentioned corrections, it has not been thought necessary to give it. As an example, the following experiment is given in detail:

Let W be the weight of the calorimeter alone.
Let $W_{\prime}$ be the weight of the calorimeter + water before adding water from vessel A.
Let $W_{\prime\prime}$ be the weight of the calorimeter + water after adding water from vessel A.
t = temperature of calorimeter before, $t_{\prime}$ = temperature of mixture.
$t_{\prime\prime}$ = temperature of water in vessel A.

Then with the proper corrections

$$\frac{(W_{\prime}-W)(t_{\prime}-t)\times\text{sp. heat between } t \text{ and } t_{\prime}}{(W_{\prime\prime}-W_{\prime})(t_{\prime\prime}-t_{\prime})\times\text{sp. heat between } t_{\prime\prime} \text{ and } t_{\prime}}=\text{constant, then}$$

$$\frac{\text{sp. heat between } t_{\prime\prime} \text{ and } t_{\prime}}{\text{sp. heat between } t_{\prime} \text{ and } t}=\frac{(W_{\prime}-W)(t_{\prime}-t)}{(W_{\prime\prime}-W_{\prime})(t_{\prime\prime}-t_{\prime})}.$$

W = with added solder 392·5; $W_{\prime}$ = 1574·8; $W_{\prime\prime}$ = 2419·9.

Zero readings of thermometer 108,947 were ·35 ·38 ·38
Zero readings of thermometer 108,954 were ·00 ·00 ·00

	Readings on Thermometer.	
	108,947	108,954
Time.	In calorimeter.	In vessel A.
42	243·39	0·00
43	242·95	0·00
44	242·50	0·00
45	242·05	0·00
		In calorimeter.
$46\frac{3}{8}$	143·50	140·60
$47\frac{3}{8}$	143·60	140·68
$48\frac{3}{8}$	143·70	140·74

Water running from time $45\frac{3}{8}$ to $46\frac{1}{4}$, therefore temperatu water in calorimeter at the instant of mixing is that at $45\frac{3}{8}$ or 241·75 and the temperature of mixture the mea 140·53 and 143·40, now

241·75 corrected +·019 for zero and + 046 for stem = 33·
143·40 corrected +·019 for zero and — 004 for stem = 19·
140·53 corrected 0·00 for zero and — 004 for stem = 19·

$$W_{,}-W=1574{\cdot}8-392{\cdot}5+35{\cdot}4+1{\cdot}1 \quad\Big|\quad t_{,}-t=33{\cdot}507-19{\cdot}73$$
$$W_{,,}-W_{,}=2419{\cdot}9-1574{\cdot}8+1{\cdot}1 \quad\Big|\quad t_{,,}-t_{,}=19{\cdot}735-0$$

hence

$$\frac{\text{sp. heat } 0^\circ-19^\circ}{\text{sp. heat } 19^\circ-33^\circ}=\frac{(1574{\cdot}8-392{\cdot}5+35{\cdot}6+1{\cdot}1)(33{\cdot}507-19{\cdot}735)}{(2419{\cdot}9-1574{\cdot}8+1{\cdot}1)\,19{\cdot}735}=1{\cdot}00$$

The temperatures throughout are on the absolute scale. results are given in a more concise form in Table I. Ir table the first two columns give the weight of the calorir and contained water before and after the addition of water the reservoir A. The next eight, of which four are de to thermometer 108,947 and four to 108,954, are occupie the readings, temperature of surrounding air, correction fo truding stem and reduced temperature (absolute scale). each experiment the initial temperatures are placed on upper row and the final temperatures, i. e. those of the ture, occupy the lower row. The ratios of the specific between the assigned limits will be found in the last tw umns, one of which is taken up by Rowland's fig Although it was attempted to make the limits coincide Rowland's, still in many cases this was impracticable blanks have in consequence been left in three or four p Where the temperature intervals are comparable it will be that there is great similarity in the results.

It may be interesting to further compare these values those which can be computed from the tables given in fessor Rowland's paper on the mechanical equivalent of The specific heat varies directly as the mechanical eq lent; it is therefore only necessary to plot a curve wit

TABLE I.

Weight of Calorimeter.			Thermometer 108,947 in calorimeter.				Thermometer 108,954, first in vessel A, afterwards in calorimeter.				Reduced ratios of the specific heats.	
Before.	After.		Reading.	Air.	Correction.	Temp'ture.	Reading.	Air.	Correction.	Temp'ture.	Liebig.	Rowland.
1609·3	1897·9	Initial	123·25	21°·0	−006	16·937	0·00	21°·0	000	0·00	$\frac{0-13}{13-17}$=1·0032	--------
		Final	100·26		−007	13·755	97·98		−007	13·768		
2017·6	2528·6	Initial	216·43	22·0	+029	29·952	0·00	22·0	000	0·00	$\frac{0-22}{22-30}$=1·0044	--------
		Final	166·04		000	22·893	162·96		000	22·878		
2129·6	2576·4	Initial	159·75	21·0	+002	22·014	0·00	21·0	000	0·00	$\frac{0-17}{17-22}$=1·0015	$\frac{0-17}{17-25}$=1·0027
		Final	127·89		−006	17·578	----		----	----		
1827·2	2358·3	Initial	206·20	23·0	+017	28·509	0·00	23·0	000	0·00	$\frac{0-21}{21-28}$=1·0030	$\frac{0-21}{21-28}$=1·0023
		Final	152·00		−004	20·935	149·11		−004	20·931		
1921·3	2654·4	Initial	183·00	22·0	+009	25·267	224·75	22·0	+024	31·521	$\frac{25-27}{27-31}$= ·9995	--------
		Final	197·25		+016	27·261	----		----	----		
1774·5	2515·1	Initial	153·00	23·0	−004	21·078	216·40	23·0	+010	30·852	$\frac{21-24}{24-31}$= ·9989	$\frac{20-24}{24-29}$= ·9983
		Final	177·00		+004	24·427	174·00		+004	24·432		
1971·1	2957·1	Initial	152·00	24·0	−004	20·939	209·60	24·0	+010	29·406	$\frac{21-24}{24-29}$= ·9980	$\frac{21-29}{29-36}$= ·9954
		Final	175·00		+004	24·148	172·00		+004	24·151		
1574·8	2419·9	Initial	241·75	22·0	+046	33·507	0·00	22·0	000	0·00	$\frac{0-20}{20-33}$=1·0053	$\frac{0-21}{21-34}$=1·0062
		Final	143·40		−004	19·741	140·53		−004	19·729		
1824·8	2234·1	Initial	220·50	22·0	+034	30·536	0·00	22·0	000	0·00	$\frac{0-24}{24-30}$=1·0040	--------
		Final	172·81		+010	23·854	169·75		+010	23·841		
1677·3	2341·5	Initial	224·63	23·0	+032	31·102	0·00	23·0	000	0·00	$\frac{0-21}{21-31}$=1·0050	$\frac{0-18}{18-30}$=1·0067
		Final	149·98		−003	20·657	147·08		−003	20·648		
1616·5	2357·5	Initial	217·25	23·0	+027	30·067	0·00	23·0	000	0·00	$\frac{0-19}{19-30}$=1·0057	--------
		Final	137·20		−007	18·876	134·60		−007	18·896		
1488·1	2130·6	Initial	262·23	22·0	+064	36·397	0·00	22·0	000	0·00	$\frac{0-23}{23-36}$=1·0034	$\frac{0-21}{21-34}$=1·0062
		Final	168·10		+002	23·183	165·05		+002	23·173		

values of the mechanical equivalent (given in table LIII) different temperatures as ordinates and temperatures as a scissæ. Then measuring off ordinates as averages between tl required limits, and dividing one by the other, the ratios the specific heats between the same degrees of temperature wi be obtained.

TABLE II.

Temperature intervals.	Mech. equivalent value.	Rowland.	Neesen.	Liebig.
$\frac{0-14}{14-17}$	1·0046	....	....	1·0030
$\frac{0-17}{17-22}$	1·0056	1·0027	....	1·0015
$\frac{0-17}{17-29}$	1·0067	1·0024	1·0079	....
$\frac{0-18}{18-27}$	1·0063	1·0025	1·0099	....
$\frac{0-19}{19-30}$	1·0065	1·0067	1·0111	1·0057
$\frac{0-19}{19-33}$	1·0066	1·0062	....	1·0053
$\frac{0-20}{20-31}$	1·0064	....	....	1·0051
$\frac{0-21}{21-28}$	1·0060	1·0045	1·0358	1·0032
$\frac{0-23}{23-30}$	1·0059	....	....	1·0043
$\frac{0-23}{23-36}$	1·0068	....	....	1·0034
$\frac{0-24}{24-29}$	1·0058	....	....	1·0045
$\frac{20-24}{24-29}$	1·0013	·9983	·9794	·9980
$\frac{21-24}{24-31}$	1·0012	....	....	·9989
$\frac{21-29}{29-36}$	1·0007	·9954	....	....
$\frac{25-27}{27-31}$	1·0004	....	....	·9995
$\frac{29-32}{32-38}$	·9996	....	....	....

A glance at table II will show how these compare with th results of other determinations. It will be remarked tha although in no case do the figures (with the exception of Ne sen's) differ very greatly among themselves, still there is a r markable discrepancy in the indications of the position of th minimum. The values deduced from the mechanical equiv lent certainly place the minimum at about 30°, while thos obtained from direct experiment as surely place it at abo

28°. There can be no doubt that experiments on the mechanical equivalent can be made with far greater accuracy than those on the specific heat by the method of mixture, and the total variation between 0° and 30° is only about one per cent; nevertheless it does appear surprising that three independent series of results (two by one method and one by another) all agreeing so closely on this point, should differ from the original value given by the mechanical equivalent. I have in vain sought its cause in any constant error which I could detect inherent in either method, and have now no explanation to offer of this minor discrepancy, but reaffirm as the result of direct experiment that the specific heat of water decreases from zero to a point between twenty and thirty degrees centigrade.

Baltimore, May 16, 1883.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

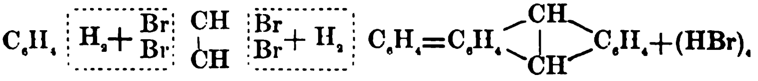
1. *On the Variability of the Law of Definite Proportions.*—A year or more ago, Schutzenberger announced that in analyzing some hydrocarbons, the sum of the carbon and hydrogen was 101 for 100 parts of material; the result under other conditions being normal. Boutlerow has called attention to this anomaly, as illustrating views he has held for three years. The result can be accounted for by supposing (1) that the absolute quantity of ponderable matter has increased, that which we call energy being transformed into that which we call matter; or (2) the quantity of matter remaining the same, its weight has increased; neither of which hypotheses are admissible; or (3) that the weight of the substance has not varied but its chemical value has changed. If the atomic weight of carbon falls temporarily to 11·8 in place of 12, for example, its capacity for saturation is increased by a sixtieth and the quantity of carbon dioxide formed by the same quantity of carbon by weight, is greater in that ratio. The author considers this hypothesis by no means inadmissible, even when the classic researches of Stas are taken into the account. Hence he has undertaken a series of experiments to test it. The atomic weights of red and of white phosphorus will be accurately compared; the quantity of $HgCl_2$ formed from a known quantity of metal, which is combined directly with chlorine either in the dark, in sunlight or in magnesium light or under a variety of influences, will be determined; and so on. Stas's labors have fixed the absolute constancy of atomic weights, it is true, but only under certain conditions. There may be other conditions under which the atomic weights and the composition of bodies will vary. Indeed if we accept the results of Stas we must admit that the atomic weights cannot be expressed in whole numbers;

but who can say, in the present state of science, that the hypothesis of Prout does not rest on a more or less solid foundation? Other experimental laws, as for example, that of Boyle and Marriotte, vary within certain narrow limits. May not Prout's hypothesis be true under conditions not yet understood? To ask such a question is to deny the absolute constancy of atomic weights. But if we put aside the atomic theory of the physicist which regards atoms as actual indivisible particles, then the atomic weight of an element represents only a certain quantity of matter by weight, the carrier of a fixed quantity of chemical energy. For other forms of energy, the quantity is far from being fixed by the mass of matter; on the contrary, the mass may be the same and the energy increase, as when the velocity increases. Why may not the same thing take place for chemical energy, although confined within certain narrow limits? At first it appears strange to admit the possibility of a variation in the composition of a definite chemical compound. Carbon dioxide, for example, will no longer be a body containing absolutely 32 parts of oxygen for 12 parts of carbon (or rather 31·92 for 11·97); it will be a combination of carbon and oxygen in which the relative quantity of the constituents may vary, for instance, between the limits 12 : 32 and 11·8 : 32. But will these varieties of carbon dioxide constitute identically the same chemical compound? Certainly up to a certain point. There will no longer be absolute identity, but the properties in general, especially the chemical properties will remain the same. In fact the properties of a compound are only the result of the reciprocal action of the elements which mutually combine with and saturate each other, this state of saturation remaining unchanged in all cases since the relative quantities of chemical energy acting on the side of each element remain the same in spite of the changes in the mass of the carriers of this energy.

After the presentation of the above paper to the Chemical Society of Paris by Wurtz, SCHUTZENBERGER gave his views on the subject, enumerating the facts which led him to the conclusion that the law of definite proportions was not absolute as generally supposed. According to his analyses, a body such as water may vary in composition between very narrow limits, the differences not sensibly affecting its properties. Between these limits, there is a ratio corresponding to the maximum stability which in the majority of cases is attained. A crystallized body, such as copper sulphate or ferric oxide, is generally constant in composition, the crystallization rigorously determining the ratio; but in most cases it is possible to vary the composition of bodies by varying the circumstances under which they are formed. Of this he gives the following cases: (1) hydrocarbons, such as are obtained from Caucasian petroleum, or even turpentine, when burned in a combustion tube with CuO and a current of oxygen, show always a loss of carbon of 1 to 1½ per cent when effected at a low temperature and under circumstances where no carbonous oxide or empyreumatic products could escape. (2) When diamond is burned at a high temperature in pure oxygen, the carbon dioxide formed has oxidizing properties which it does not possess when produced by the combustion of an organic compound at the expense of CuO. (3) Barium carbonate obtained by precipitating baryta water pure, boiling, by an excess of CO_2, washing and drying at 100°, then at 440°, contains, as Berzelius showed, 21·7 per cent CO_2 for 78·5 per cent BaO. Heated to a red heat in a current of dry oxygen, it increases considerably in weight without losing CO_2; and the product gives 22·0 to 22·05 of CO_2 to 76·5 of BaO. (4) In calcining cream of tartar at a red heat and lixiviating, a solution is obtained which, on the addition of boiling baryta water, gives a homogeneous crystalline precipitate in fine needles, drying to a yellowish powder, increasing in weight and becoming white by calcination in oxygen. Before calcination, it contained 21·6 CO_2 to 76·5 BaO; afterward 22·05 to 76·5. The potassium carbonate from the black flux, previously heated to redness in a current of oxygen, gives with baryta water in ebullition a white amorphous precipitate, with a tint on drying rather blue than yellow, containing 22·0 CO_2 and 76·5 BaO. (5) When copper oxide is reduced by CO, the ratio of the CO to O in place of being 14:8 is 14:8·05; while if the reduction is effected by hydrogen the ratio is 1:7·96. One equivalent of CO can therefore remove 8·05 of O from CuO at a dark red heat; while one equivalent of hydrogen removes at the same temperature only 7·96. If the equivalent of O be 8, then that of CO is 13·91 and that of C 5·91. (6) When water is synthesized by reduction of a known weight of CuO, by weighing the reduced copper and the water formed, it is found that the ratio of O to H is not constant, but varies with the state of division and of saturation of the oxide, the duration of contact of the water formed with the oxide, and with the temperature, from 7·95 to 8·15. The latter value is obtained with a saturated and divided oxide filling the tube; the former with oxide in lumps filling the tube for a space of 25 cm. With a larger empty space, the ratio has fallen to 7·90. (7) When the synthesis of water is effected by weighing the hydrogen consumed (as by dissolving a known weight of zinc in HCl) and the water formed, the ratio differs according to the contents of the combustion tubes. If it contains granular CuO over a length of 80 cm. heated to redness, the ratio of O:H is 7·96 to 7·98:1; at a low temperature, 7·90 to 7·93; if the CuO is replaced by $PbCrO_4$, from 7·89 to 7·93. (8) Whenever the ratio in these syntheses is above 8, the water formed, while neutral, possesses oxidizing properties analogous to those which the sur-oxygenated CO_2 possesses, and distinct from those of hydrogen peroxide. (9) Numerous analyses of metallic oxides show the same variation in composition within narrow limits, according to their mode of formation. HgO produced from the nitrate produces, in oxidizing formic acid, more carbon dioxide than the precipitated oxide. SnO_2 from calcined metastannic acid contains more oxygen than that produced by oxidizing the lower oxide. MnO_2 prepared by calcining the nitrate contains more oxygen than the formula requires. Ferric oxide obtained from the nitrate, gives the atomic weight 54 for Fe, from the formula Fe_2O_3; while the ferric oxide obtained by roasting ferrous oxalate gives 56. Lead oxide, when produced by calcining the nitrate gives for lead an atomic weight of 207·2; while that obtained by calcining the carbonate in nitrogen, contains less oxygen. The same differences are observed with cadmium, zinc, and copper oxide.—*Bull. Soc. Ch.*, II, xxxix, 257, 263, Mch. 1883. G. F. B.

2. *On Platinized Magnesium as a Reducing Agent.*—**Ballo** has called attention to the fact that magnesium, which has absolutely no action upon pure water, decomposes this liquid rapidly with the evolution of hydrogen and the production of magnesium hydrate, on the addition of a few drops of platinic chloride. He therefore recommends platinized magnesium as a reducing agent. Some nitrobenzene, mixed with a few cubic centimeters of water and enough alcohol to bring it into solution, was treated with magnesium; upon the addition of a few drops platinic chloride, action began and a feeble evolution of gas appeared. The liquid filtered from the excess of magnesium and the hydrate, contained aniline, the reduction being complete. For the detection of nitrobenzene this method has the advantage that the base is produced in the free state.—*Ber. Berl. Chem. Ges.*, xvi, 694, Apr. 1883. G. F. B.

3. *On a New Synthesis of Anthracene.*—In the hope of producing an isomeric tetraphenylethane, **Anschütz** and **Eltzbacker** studied the action of aluminum chloride upon tetrabromide of acetylene dissolved in benzene. From the products of the reaction, a hydrocarbon was easily isolated, difficultly soluble in the ordinary solvents, which instead of the expected tetraphenylethane proved to be anthracene. The reaction may be represented as follows:—



$$C_6H_4\,\boxed{H_2+\begin{matrix}Br\\Br\end{matrix}}\;\begin{matrix}CH\\|\\CH\end{matrix}\;\boxed{\begin{matrix}Br\\Br\end{matrix}+H_2}\;C_6H_4=C_6H_4\begin{matrix}CH\\|\\CH\end{matrix}C_6H_4+(HBr)_4$$

The importance of this synthesis is evident. In the first place it affords the first experimental evidence for the assumption generally made that the middle carbon atoms in anthracene are directly united. And in the second, it is the first aluminum chloride reaction in which a single benzene molecule gives up two hydrogen atoms to unite with a hydrocarbon residue.—*Ber. Berl. Chem. Ges.*, xvi, 623, April, 1883. G. F. B.

4. *On the Preparation of Acetol from Sugar.*—Acetol is the alcohol of acetone. **Emmerling** and **Loges** have prepared it from both dextrose and saccharose. Anhydrous dextrose is fused in a retort and half its weight of caustic potash added in small pieces. With active evolution of empyreumatic fumes, a liquid distilled over which reduced Fehling's solution in the cold. On fractioning, a combustible liquid was obtained boiling at 80°. But the dehydrating process is difficult; so that the presence of the acetol was

proved: 1st, by oxidation with chromic acid which gave acetic and carbonic acids; and 2d, by reducing copper oxide with it. Saccharose also yields acetol by this treatment.—*Ber. Berl. Chem. Ges.*, xvi, 837, April, 1883. G. F. B.

5. *On the Product of the slow Combustion of Æther.*—The peculiarly penetrating odor produced by the slow combustion of ether has been examined by LEGLER. From 150 to 200 c. c. of ether, 25 to 30 c. c. of the crude product was obtained. This exposed for ten days over sulphuric acid gave crystals in rhombic prisms resembling ice flowers, soluble in water, alcohol, ether and chloroform, fusing at 51°, neutral in reaction and affording on analysis numbers leading to the formula $C_{11}H_{22}O_{11}$. With alkalies it evolves hydrogen, formic acid and methyl aldehyde. With ammonia, it gives oxygen with the other bodies.—*Liebig Ann.*, ccxvii, 381, April, 1883. G. F. B.

6. *Curved diffraction gratings.*—Mr. R. T. GLAZEBROOK enters into a mathematical investigation to show that the concave gratings of Professor Rowland are subject to aberration effects. He shows that concave gratings may have too large a surface, and that better effects can be obtained by covering over a certain portion of the ruled surface. He finds that in one grating "if its size be reduced to $\frac{3}{8}$ of what it actually is, the extreme lateral aberration will be reduced to about $\frac{1}{4}$ of its actual value, thus becoming about $\frac{1}{350}$ of a centimeter, and the extreme difference of phase in the light of a given wave-length λ reaching any point of the diffracted spectrum will never exceed $\lambda/4$, the dispersion will remain unaltered, the definition and the brightness of the spectrum will both be increased." With lenses of focal length which are indicated, Glazebrook shows that a plane grating ruled on glass, would in certain cases give more light for good definition than a concave grating.—*Phil. Mag.*, June, 1883, 414–423. J. T.

7. *Regenerative theory of Solar action.*—Under this title Mr. ERNEST H. COOK criticises Siemens' theory of solar energy. No evidence is found in planetary atmospheres of a difference in composition at high and low altitudes. Gay-Lussac and Thénard collected air in a balloon at an elevation of 7,000 meters and found that it contained exactly the same proportional quantity of oxygen as that collected at the same time in Paris. Brunner analyzed the air collected at the top and at the bottom of the Faulhorn and found the same proportions of oxygen and nitrogen. Frankland has found that the composition of air up to altitudes of 14,000 feet is constant. There is, therefore, no terrestrial evidence of the separation of the constituents of the atmosphere which the theory supposes. Again the evidence for the theory deduced from the composition of meteorites is not conclusive. In certain meteorites a very large proportion of carbon dioxide is found—much larger than exists in our atmosphere. This would seem to indicate that the meteor had passed through an atmosphere very rich in carbon dioxide. F. Mohr has shown that

a large proportion of water is contained in the porous structure of meteorites, and he and Prof. Smith have also detected the presence of carbonaceous substances in meteoric bodies. When the meteor is raised to a high temperature by coming into contact with our atmosphere—the water of constitution would be decomposed, the hydrogen being set free and the oxygen would then combine to form magnetic oxide. The fused shell of the exterior of the meteorite would thus enclose gases at a high pressure. Since all the planets are also immersed in the supposed atmosphere—of Dr. Siemens, an ærial current should form constantly in the northern hemisphere from the northeast, and in the southern from the southeast. These currents would be opposed to the "return trades." Mr. Cook discusses the dissociation effects in the atmosphere of the sun, and finds that the constitution of the gases projected into space would be very different from that supposed by Dr. Siemens. The spectroscopy of the stars moreover affords no evidence of Dr. Siemens' hypothesis.—*Phil. Mag.*, June, 1883, pp. 400-405. J. T.

8. *Effect of Pressure on the melting point of ice.*—The experiment of Bottomley, which apparently illustrates the fact that increased pressure lowers the freezing point of water, is well-known. A wire weighted at both ends is thrown over a cake of ice and cuts through it. The ice melts beneath the wire and freezes above it. Professor Guthrie, at a meeting of the Physical Society held in London, April 28th, 1883, gives his reasons for believing that the wire conducts heat to the ice from the atmosphere and that the experiment does not illustrate the well-known fact that increased pressure lowers the freezing point. A silk wire, weighte to the same amount as a metallic wire, will not cut through block of ice. J. T.

9. *Electro-technische Bibliothek;* Vienna, 1883 (A. Hartleben Verlag): II, Die elektrische Kraftübertragung und ihre Anwendung in der Praxis, mit besonderer Rücksicht auf die Fortleitung und Vertheilung des elektrischen Stromes, dargestellt von Eduard Japing. 239 pp. 12mo.

III, Das elektrische Licht und die hierzu angewendeten Lampen, Kohlen und Beleuchtungskörper, dargestellt von Dr. Alfred von Urbanitzky. 223 pp. 12mo.

IV, Die galvanischen Batterien Accumulatoren und Thermosäulen, eine Beschreibung der hydro- und thermo-elektrischen Stromquellen mit besonderer Rücksicht auf die Bedürfnisse der Praxis von W. Ph. Hauck. 320 pp. 12mo.

XI, Die elektrischen Beleuchtungs-Anlagen mit besonderer Berücksichtigung ihrer practischen Ausführung, dargestellt von Dr. A. von Urbanitzky. 240 pp. 12mo.—The first volume of this valuable series of works on the practical applications of Electricity was noticed in the February number, and the special design of the series was there alluded to. The four volumes recently received cover some of the most important of the topics, and their scope will be gathered from the titles which are given above in the

The individual volumes are small, and yet they contain a large amount of information compressed into a small space. For example the volume on the Electrical Light, after the discussion of the theory of the incandescent and arc light and the division of the electrical current, the different kinds of lamps are described under five heads: first, the incandescence lights based upon imperfect conductive power; second, those with imperfect contact; third, the arc lights which are regulated by an electro-magnet; fourth, the electrical candles; fifth, the lamps with inclined carbons. Under these five heads, some 80 forms of lamps are described; most of them with the assistance of excellent figures. Volume XI of the series, by the same author, discusses the remainder of the subject, that is the production and distribution of the electrical current used to produce the light, and the various other related topics. Volume II on the distribution of energy of the electrical current, and volume IV, on galvanic batteries, contain a clear, and in so far as the space allows, satisfactory discussion of these subjects.

II. GEOLOGY AND MINERALOGY.

1. *Geological unification.*—The Swiss "*Comité d' unification Géologique*, of which Prof. Renevier is President, held its third meeting at Berne, on the 9th of April last, and was occupied with a discussion of questions submitted to it in a circular from Professor Capellini of Bologna, President of the last Congress. These questions are to be acted upon at the meeting at Zurich in August next. The points of widest interest are the following:

(1) In the scheme for the chart of Europe, which has 27 stratigraphical subdivisions, the committee proposes to have the number of divisions of the Cretaceous made *three* instead of two, and the Permian to have but *one* in place of two. The Gault and Cenomonian make the middle Cretaceous.

(2) The uniting of the Rhetian to the Lias is recommended, to which in its rocks and fossils it is allied. In Switzerland the Rhetian is the first marine fossiliferous group after the Triassic which is ordinarily without fossils.

(4) The separation of the Flysch from the Eocene and its union with the Oligocene would be impossible in the larger part of the Swiss Alps. The Nummulitic beds are often interstratified with the Flysch.

(5) The Silurian group with its three divisions, of which the lower corresponds to the Cambrian, should be represented on the map by a single color, of three different shades, as recommended recently by Professor Hebert before the Geological Society of France, in a paper aiming to prove that the Cambrian should be attached to the Silurian as its lowest subdivision. The view corresponds to Barrande's three Silurian faunas, the *first*, *second* and *third*. [This recommendation with reference to the Silurian accords with the facts in North America. A separation of the Cambrian from the Silurian, beyond what is above proposed, is

not sustained by any sufficient paleontological or stratigraphi
evidence.]

The Carboniferous also should have its three subdivisions,
Culm (Subcarboniferous), the Carboniferous and Permian; and
also the Devonian.

A *gray* color is recommended for the Carboniferous; an *ol*
green for the Devonian, and *brown* for the Silurian.

(7) The term *Series* should be used for the highest subdivisi
and *Group* for the third in grade. Group was suggested for t
higher at Bologna (see this Journal, xxiii, 151, 1882).

The committee considered also other points. It regards f
shades of yellow for the Tertiary as more than can be made c
tinct, and recommends four. A bright yellow is recommended
the Drift, or Diluvian deposits, and the same dotted for the Plioc
which "ne sont la plupart du temps que les formations mari
contemporaines des dépôts erratiques anciens, dits quaternai
ou diluviens."

The grouping with reference to eruptive rocks proposed
Bologna is objected to.

The division "porphyritic rocks" is based, it states, only
mode of texture, while, in other cases, the reference is to chemi
or mineralogical characters. Some porphyries graduate into gr
ite and others are petrographically melaphyre or basalt, a porph
ritic structure being of secondary importance. The division *vol*
nic rocks is objected to, it including trachytic as well as basal
kinds. The Swiss committee proposes the two primary divisic
of *recent* and *ancient* eruptive rocks; and, under these, the t
groups of *acidic* and *basic*. For modern volcanic areas, where t
lavas are often of different kinds, variously mingled, a separ
color is recommended.

The five colors for the eruptive areas should be some tint
red; and those proposed are *carmine red* for ancient acidic er
tive; *purplish red* for ancient basic; *scarlet red* for recent acidi
and *brownish red* for recent basic; *minium red* for modern er
tions.

For the Archæan the committee proposes three shades of *ro*
for the Trias, three of *violet;* the Rhetian to oolite, inclusive, *blu*
Cretaceous, *green;* in each case, the older of the subdivisions t
deepest in shade of color, and if there are four subdivisions in a
case, using dots for the lowest, in addition to the color. J. D. D.

2. *Bulletin No. 3 of Princeton College Museum.*—This nu
ber of the Princeton Bulletin contains the following papers: (
on the Skull of the Eocene Rhinoceros, *Orthocynodon*, and t
relations of this genus to other members of the group, by Prof
sors W. B. SCOTT and H. F. OSBORN; (2) on Achænodon,
Eocene Bunodont, by H. F. OSBORN; (3) Observations on t
Brain Casts of Tertiary Mammals, by ADAM T. BRUCE; on D
mutotherium and Dilophodon, two new Eocene Lophiodonts,
W. B. SCOTT. The last paper is finely illustrated by plates V, V
VII, VIII (three of them folded), in lithography.

Mr. Bruce's paper on Brain casts of Tertiary Mammals closes with the following conclusions:

In conclusion it seems proper to point out some phases of encephalic growth which the casts described suggest. When comparison was possible, the casts, after making due allowances for the minor features of the brain which cannot be well represented by a cast, were found to be smaller and less highly convoluted than the brains of existing mammals of the same size and order. The casts of Oreodon were much smaller than the pig's brain: making deductions for the smaller size of Oreodon, which the numerous remains of its skeleton amply prove, it appears that the brain of the pig is relatively larger. The brain cast of Poëbrotherium does not admit of any exact comparison with the brains of existing Camelidæ.

The brain cast of Megencephalon, though smaller than the sea-otter's brain, does not admit of any exact comparison with the brain of the otter, in the absence of the cranial bones. The brain cast of Hoplophoneus indicates a relatively small-sized brain. Though probably as large as a panther, this animal had a brain not much larger than that of a cat.

As before intimated, the brain of Leptictis must have been small considering the size of the animal. In all the casts where it is distinct, the cerebellum is nearly or entirely uncovered by the hemispheres. Moreover the cerebellum is, in most instances, of a relatively large size. Consequently, the evidence presented by the specimens described confirms the statement of Professor Marsh, that most Tertiary Mammals had small brains, and that encephalic growth has been mainly confined to cerebral growth.

It seems probable that the primitive gyri on the dorsal surface of the hemispheres were ill-defined, and of the longitudinal type, running parallel to the dorsimeson with little flexure. The fact that the brains of Rodentia, Edentata and Insectivora generally present this type of gyri, is evidence that the least differentiated gyri were arranged as stated. The brain casts of the Carnivora described, with the exception of *Megencephalon primævus*, exhibit nearly straight longitudinal gyri, not diverging from the meson anteriorly or posteriorly, as do the corresponding gyri on the cerebra of modern Carnivora. Moreover, the transverse crucial fissure prominent in existing Carnivora, if present at all, must have been quite rudimentary. * *

The large size of the cerebellum in fossil mammals has been pointed out. However, it does not necessarily follow from this that there has not been an increase of cerebellar cortex effected, as in the cerebrum, by the folding of the surface. The most noticeable feature of the cerebellum of the casts examined is the prominent middle lobe. Existing mammals of the lower orders possess a similarly configured cerebellum. In man the middle lobe is reduced to a minimum, the lateral lobes comprising the greater part of the cerebellum. It seems probable that there has been a decrease in the size of the middle lobe and a concomitant increase of the lateral cerebellar lobes with progressive evol tion.

Moreover, the supposition that the middle cerebellar lobe the Mammalia was primarily the largest cerebellar lobe is su ported by embryological evidence, that lobe being prominent i the embryonic phases of the higher Mammalia, where it subs quently becomes smaller than the lateral lobes.

3. *Die Vergletscherung der deutschen Alpen, ihre Ursache periodische Wiederkehr, und ihr Einfluss auf die Bodengesta tung.* [*The Glaciation of the German Alps; its causes, period cal recurrence, and its influence on surface-configuration.*] *G krönte Preisschrift*; von Dr. Albrecht Penck. pp. viii + 48 with 16 wood-cuts, 2 maps and 2 tables. Leipzig, Barth, 1882.— This important treatise is divided into three sections, viz: I. Th last glaciation of Upper Bavaria and North Tyrol; II. Olde glaciation of Upper Bavaria and North Tyrol; and III. Th formation of the Upper Bavarian lakes. In addition it include introductory and concluding chapters, consisting respectively c an admirable résumé of the history of glacial geology, and thorough critical discussion of current theories of glacial climat

In the second section, which is of greatest general interest, i is shown that the phenomena of the Bavarian and Tyrolese Alp afford decisive evidence, coming from diverse independent source of two widely separated epochs of extensive glaciation, with mor equivocal evidence of an intermediate epoch of less energetic ic work. Thus, the Höttingen breccia (hitherto regarded as Miocen from the character of its plant remains) is a type of certain ob scure formations of the northern Alps, which have long bee known to be unconformably overlain by glacial deposits. No Penck finds that this breccia at Höttingen (near Innsbruck) i simply an ancient alluvial deposit, and moreover that it repose unconformably upon older glacial deposits. It accordingly ind cates the following vicissitudes during the *a*-glacial period whic it represents: (*a*) The formation of an extensive alluvial deposit (*b*) the cementation of its materials into a compact breccia; (erosion of the breccia and concurrent accumulation of stream all vium in places; (*d*) considerable changes in the configuration the valley, and excavation of the stream-channel below the leve of the breccia, and (*e*) accumulation of the basal stratified bed (unterglacialschotter) of the second ice-epoch to a depth of 200 (p. 238). Like relations among analogous formations occur els where. From the assemblage of phenomena the historical s quence is thus interpreted:

1. ***First glaciation*** and retreat of the ice.
2. Formation of a great alluvial deposit; clothing of the vicinit of the Inn valley by vegetation; cementation of the depos into the Höttingen breccia of to-day; erosion of the brecc and considerable deepening of the Inn valley.

[illegible]robable re-distribution of the rocks of the central Alps, o their northern flanks, perhaps by a *second glaciation*.

4. Accumulation of stream detritus nearly to the base of the Höttingen breccia; re-excavation of the Inn valley to its previous depth.
5. Advent of the *last glaciation;* deposition of the laminated clays of Arzl, and of the coal of Mühlau; accumulation of the basal glacial deposits overlying the Höttingen breccia; development of moraines on the heights, and of terraces on the valley sides, with retreat of the glaciers.
6. Erosion of the Inn valley; excavation of ravines within and deposition of alluvial beds upon the terraces (p. 243).

Thus, again, a typical section of the "inter-glacial" coal of the Iller valley, near Imberg, exposes a ground moraine 40–60 m. thick resting upon a cemented conglomerate of local materials, which, with the included members, reaches a thickness of fully 25 m. Within this conglomerate occur two coal-seams respectively ½ m. and 2 m. thick, and beneath the coal, a bed of laminated clay some 8 m. thick, containing flattened sticks and twigs. Below all, lies a ground moraine, filled with scratched bowlders and pebbles, 10 m. thick. This sequence and the coincident phenomena in various parts of the Iller valley indicate the following course of events:

1. The valley became filled with glaciers which, in the vicinity of Sonthofen, descended to within 900 m. above sea level. Temperature low.
2. The glaciers withdrew; gravel-beds attaining a thickness of 60 m. were deposited. Elevation of temperature.
3. Vegetation extended over the newly-formed gravel surfaces. Its remains accumulated in two coal-seams whose maximum thickness exceeds 3 m.; which accumulation of vegetal matter could only have taken place during a long period. The coal-seams became covered with gravel-beds.
4. The Iller valley was excavated to a depth of 210 to 220 m. Temperature remained high.
5. The glacier expanded a second time. The temperature again fell (p. 261).

Similarly, the protean deposit long known as "Alpine diluvium" is essentially bipartite in structure. The upper and newer division, which is of unconsolidated water-washed materials, appears to have been the precursor of the last glaciation and is intimately connected with its moraines (the term *unterglacialschotter*, by which it is conveniently designated, has no English equivalent). The lower and older division—the "diluviale Nagelfluh"—is of flood-borne materials which were cemented into a firm conglomerate and superficially weathered into a red-brown clay before the last glaciation; and its surface is locally striated and overspread by moraines. Extensive valley-excavation and mountain-carving everywhere intervened between the eras of deposition of the two divisions; and in some localities these processes were interrupted by a short period of dispersion and deposition of high-

Alpine materials. Finally, of the two distinct zones of moraines which flank the Bavarian Alps, the outermost is unconformably overlain by stratified loams and clays, löss, and other peripheral representatives of the newer "diluvium," which must have been deposited contemporaneously with the moraines of the inner zone; which moraines are without such covering. Alike from the breccias, from the "inter-glacial" coal, from the flood deposits, and from the moraines of the Alps, then, the recurrence of eras of glacial climate is determined; and in each case the intervals separating the successive ice-periods are shown to have been far too great to be explained as results of temporary oscillation of the ice-sheet, or even of Crollian inter-glacial periods.

In the third section it is shown that the glacial deposits of the Bavarian plateau are equivalent to a layer 36 m. in average thickness over the whole area whence the materials were derived; yet, since even this enormous volume is far less than the total content of the excavated depressions of the area, it appears that the configuration of this portion of the Alps must be due to pre-glacial stream-work, and that glacial erosion must have only served to modify the preëxistent topography. It is further shown by detailed consideration of the phenomena of individual cases that most of the larger lakes of upper Bavaria occupy basins scooped out by glaciers, and the multifarious objections to the general hypothesis of glacial excavation of lake-basins are discussed at length, and as applied to the region in question, proven to be mainly invalid; yet so catholic are the author's opinions that he questions whether ice is a more effective engine of erosion than water, and limits glacial action to the widening of stream-cut valleys and the deepening of preëxistent concavities. The smaller Bavarian lakes are shown to generally lie in local depressions in the irregular drift-surface.

After thorough consideration of current glacial theories in the light of the phenomena of the area specially described, of different regions previously visited by the author, and of other lands, the theory of glacial climate enunciated by Wallace in "Island Life"* is unreservedly adopted.

Appendices contain details as to altitudes at which erratic material occur, and directions of glacial striæ. Table I is a conspectus of the classifications of Alpine glacial deposits hitherto proposed, and table II is a systematic stratigraphical and historical conspectus of the Quaternary phenomena of the Alps. Map I represents the area described, and map II the present and former extent of the glaciers of the earth.

The work has already been fully reviewed by Richthofen,† and has been noticed at length in the Geological Magazine.‡ In view of the thorough, liberal and comprehensive treatment of the different phases of the subject taken up, the present writer is impelled to endorse the opinion expressed in the last-mentioned review, that "to all who are interested in the study of glacial geology we can recommend this treatise as the most important contribution on the subject which has appeared of late years."

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* See Mr. W. J. McGee's paper in the following number of this Journal.

† Verh. Erdk., Berlin, 1882, 565–577.

‡ II, x, 1883, 177–183.

W. J. MCG.

4. *Dinosaurian of the Laramie or Lignitic group.*—In the American Naturalist for July, 1883 (p. 774), Professor Cope has a paper on "the Structure and Appearance of a Laramie Dinosaurian," from a nearly perfect skeleton, found by Messrs. Wortman and Hill, in Dakota, of the *Diclonius mirabilis* of Leidy, a species closely related to the *Hadrosaurus Foulkei.* He states that the total length of the skeleton is 38 feet; that of the skull 1·18 meters. The skull is much "like that of a goose." The dentition is remarkable for its complexity and for the difference between the superior and inferior series; it includes more teeth than the Hadrosaurus, there being 630 in each maxillary bone, and 406 in each splenial bone, making in all 2,072. Dermal or corneous structures have left distinct traces in the soft sandstone about the end of the beak-like muzzle. The affinity of the Dinosaurs to birds is confirmed by the skeleton, but Professor Cope remarks that it is empirical rather than essential and is confined to a few points, as the form and position of the vomer, the large development of the premaxillary bone and the toothless character of this bone and the dentary. Professor Cope says that so far as he is aware, "the extension of the premaxillary above the maxillary, so far as to overlap the lacrymal, is unique among Vertebrata."

5. *Wyoming Historical and Geological Society.*—This society has recently published a list of Paleozoic fossil Insects of the United States and Canada, with full references and synonymy, by R. D. LACOE. 20 pp. 8vo.

6. *Sapphire from Mexico;* by GEORGE F. KUNZ. (Read before the New York Acad. of Sciences, April 30, 1883.)—While examining a lot of rolled pebbles of jasper, agate and chalcedony, that were found near San Geronimo, Estada de Oaxaca, Mexico, near the Isthmus of Tehuantepec, and brought to me, for examination as to their gem value, by Mr. Knight Neftel, I was surprised to find a rolled pebble of sapphire (corundum). It had a mottled blue and yellowish-white color; it was slightly fissured, and translucent, and did not show any crystalline form, although the rolling seemed to have brought out the cleavage more distinctly, this reflecting a pearly luster. The specimen weighed 19·223 grams. The specific gravity, 3·9002, is exceptionally low, but this may be due to the impurities in the veining. From this single pebble it would be impossible to decide whether gems occur at this locality, for similar pebbles occur in the gem regions of Ceylon, although elsewhere no gems are found; further investigation will determine this. It is not impossible that corundum may exist there in large quantities and thus prove of commercial value.

7. *Explanatory Note concerning "triclinic pyroxene"*; 1 WHITMAN CROSS.—In the Bulletin No. I of the U. S. Geologic Survey, entitled: "On Hypersthene-Andesite and on Triclir Pyroxene in Augitic rocks," which has recently been publishe and of which an abstract appeared in this Journal for Februa last (p. 139), I took the ground that a part of the pyroxene us ally considered as augite in many rocks, must, from its optic properties, be referred to the triclinic system. Since the public tion of the Bulletin Mr. Jos. P. Iddings of the U. S. Geologic Survey has very kindly called my attention to the exhaustive i vestigations of Fouqué and Levy ("Mineralogie Micrographique p. 349) upon the optical properties of monoclinic pyroxene, which they show that the ellipsoid of elasticity is so situated to produce very great variations in optical behavior in sectio which are but little inclined to each other. This is especially tl case for those sections which vary but a few degrees in positi from the one normal to the vertical crystallographic axis. careful consideration of the matter in the light of the calculatio of Fouqué and Levy, has convinced me that a great majority the instances cited in the Bulletin as indicating a triclinic pyro ene, are explainable as augite, and that the few cases which st seem anomalous are not in themselves sufficient to justify a refe ence to the triclinic system.

In explanation of this error it may be said that the work l Fouqué and Levy was not accessible to me at the time the artic on "triclinic pyroxene" was written, for, although only recent published the manuscript was completed and had been sent in f publication long before.

In regard to hypersthene-andesite I may add that it has be recently found in abundance among the volcanic rocks of Southe Colorado. Also, that the hypersthene has been isolated fro three typical "augite"-andesites of Hungary, viz: from Bagony Bath, and from the Tokajer Berg. The analyses of these hyp sthenes, although not yet ready for publication, show them to l fully normal and preclude the possibility of an admixture augite in the substance analyzed.

8. *The Minerals of New South Wales*; by ARCHIBALD LI ERSIDGE. 137 pp. Sydney, 1882.—This valuable monograph the mineralogy of New South Wales forms a part of a volun published by the Department of Mines. This volume contai also a report by Harrie Wood on the Mineral Products of Ne South Wales, and Notes on the Geology of New South Wales l C. S. Wilkinson; also a catalogue of works, papers, etc., on tl Geology, Paleontology and Mineralogy of Australia and Tasm nia by R. Etheridge, Jr. and Robert Logan Jack. The paper l Professor Liversidge contains a description of the minerals of tl country, arranged in order, with numerous analyses; a list of tl mineral localities is also given. The occurrence of gold in Ne [illegible]th Wales is described with especial fullness; some interesti [illegible]s are given in regard to the finding of large nuggets, in whi

respect, however, Victoria is far beyond New South Wales; the average fineness of the gold is stated to be 93½ per cent gold and 6 per cent silver; in Victoria, on the other hand, it is 96 gold and 3½ silver, while to the north, in Queensland, it is only about 87½ gold and 12 silver. The analyses of a large number of different kinds of coal are also given.

III. Botany and Zoology.

1. *Monographia Festucarum Europæarum*, auctore E. Hackel. Cassel and Berlin. Th. Fischer, 1882, pp. 216, tab. 4. 8vo.—A noteworthy monograph, by an Austrian professor (in the Gymnasium at St. Poelten, Austria) who has already made his mark as an Agrostologist. The first part of the work, in German, is morphological and histological; and to this belong the excellent plates, illustrating the innovation, the structure of stem and leaves in magnified sections, etc. To which is added an instructive article on the grade of "species-building," cultural investigation, hybridity, plan of taxonomical arrangement, etc.; another on geographical distribution, and a glance at the probable genetic origination of the European *Festucæ*. The second part, of nearly 150 pages, is the "Descriptio Specierum." Hackel excludes *Vulpia* from his genus, and distinguishes *Festuca* from *Poa* especially by the hilar macula being linear or linear-lanceolate and of nearly half the length of the caryopsis, instead of punctiform. Some idea of the thoroughness of the author's treatment, of his views of species, and of his particularity in regard to varieties, may be gathered from his work upon the first species of his monograph, namely *F. ovina*, L., "sensu amplissimo," as he receives it. It is the type of his section *Ovinæ*, and the subsection *Intravaginales*, where it is associated with eight other species, all of the old world. It includes nine subspecies, the first being *eu-ovina*, the last *brevifolia*, which is peculiar to Greenland and arctic-alpine North America. Eu-ovina comprises eight varieties, with names (among which are *duriuscula* and *glauca*); its var. *vulgaris* has five named subvarieties, the var. *duriuscula* has seven, etc. *Festuca rubra*, L., the type of the *extravaginales*, rejoices in six subspecies, some of which run to seven varieties, and some of the varieties run to as many subvarieties. Very thorough work, certainly; and much better for the student, as well as truer to nature, no doubt, than the raising of all or most of these forms to the rank of species. A. G.

2. *Atlas de la Flora des Environs de Paris*, par MM. E. Cosson et Germain de Saint Pierre.—This second edition (published by Masson, 1882, in 8vo), comprises 659 figures on 47 copper-plates; the older plates, as is well-known, were by one of the authors and by Riocreux; the five new ones are by Cusin. All are admirable; and to this edition two pages of letter-press are given to each plate. A. G.

3. *Compendium Floræ Atlanticæ*, par E. Cosson.—This Flora

of the Barbary States (of Tunisia and Morocco as well as Algeria), to which Dr. Cosson has devoted so many years of labor, is now getting forward. The first half of the first volume, issued in 1881, with elaborate maps, is devoted to History and Geography. Under the first head is a full account of all the botanists who have collected in the Barbary States, or have written upon their botany,—beginning with Tradescant, in 1620, and, among the rest, giving a very interesting account of the Berber Ibrahim, and of the Rabbi Mardochee, who have essentially helped in the knowledge of the botany of Morocco, collecting where no European could venture. Under the head of Geography is a full gazetteer of the names of places and stations, a geographical bibliography, an original sketch of the regions, the cartography, etc.

4. *Illustrationes Floræ Atlanticæ*, by the same author, Fasc. I, with 25 plates, in imp. 4to, appeared last autumn. The plates are capital lithographs, from drawings by Cusin. They illustrate new *Ranunculaceæ*, *Papaveraceæ* and *Cruciferæ*, and are accompanied by 32 pages of letter-press of the same size. With all this in progress, the indefatigable author is again in the field of exploration in Algeria. A. G.

5. *Systematic Census of Australian Plants, with Chronologic, Literary, and Geographic Annotations;* by Baron FERDINAND VON MÜLLER. . . . Government Botanist for the Colony of Victoria. Part I. Vasculares. Melbourne, 1882. pp. 152, 4to.—Another token of F. von Müller's wonderful industry and perseverance. By the use of small type, which allows nearly eighty lines to the page, and by abbreviated references, we are here supplied with a systematic list of all the Phænogamous and Vascular Cryptogamous plants of Australia, with full reference to authority and place of publication, geographical distribution in Australia; and with much fuller references to the authority for the genera than the Flora Australiensis affords. The name of the author is generally printed without abbreviation, his work cited down to volume, page, and figure. Great pains are also taken to trace generic names to their original. When such names have come down from the herbalists or from the ancients, they are cited thus: "*Clematis*, Linné, gen. pl. 163 (1737) from l'Ecluse (1576)." *Anemone*, Tournefort, inst. sci. herb. 275, t. 147 (1700), "from Hippocrates, Theophrastos and Dioscorides." Useful learning, certainly, but some would say in a good degree wasted upon a catalogue of this kind. And why "Linné," after a fashion of the French zoologists? *Linn.* or *Linnæus* is surely to be preferred.

It seems to us that more is lost than is gained by the intercalation of the monochlamydeous orders among the Polypetalæ; but whatever our theoretical opinion, considering that the Flora Australiensis and the new Genera Plantarum are just completed upon the ordinary model, we should have spared the Australian student the trouble which the present work is likely to give him in this regard. On the same grounds we would have held *Chori-*

petalæ and *Synpetalæ*, and still more *Calyceæ* and *Acalyceæ*, in abeyance. All this is, in a good degree, a matter of taste and propriety, in respect to which a botanist who has done and is doing so much, so single-handed, and so disinterestedly for the flora on the Australian continent, may be allowed to have very much his own way, subject, of course, to differences of opinion. A. G.

6. *Dredgings of the Steamer "Blake."*—The Bulletin of the Museum of Comparative Zoology, Vol. X, No. 5, is occupied with a Report on the Fishes, by G. Brown Goode and Tarleton H. Bean. April, 1883.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *American Journal of Science and Arts.*—In a notice of the decease of Professor Kirtland in volume xv of this Journal (1878), the remark was made that he was the last of its original subscribers. Dr. Isaac Lea, of Philadelphia, has recently sent us copies of his early correspondence with Professor Silliman, in 1818, with regard to the establishment of the Journal, together with a list of subscribers—all members of the Academy of Natural Sciences—which he obtained for him before the first number was issued. As the facts have more than a private interest, we here publish the list of names obtained by Dr. Lea, in response to Professor Silliman's letter to him, dated New Haven, April 3, 1818—which reads as follows:

"Sir: Will you pardon the liberty I take in requesting your kind countenance and favor for the Journal and your contributions to its pages, provided the plan meets your approbation." (Signed) Very respectfully, your much obliged B. SILLIMAN.

An account of the plan accompanied the letter—in nearly the form in which it was afterward published in the first number of the Journal.

The list of subscribers is as follows:

ISAAC LEA,
EDWARD CLARK,
C. D. MEIGS,
JACOB PEIRCE,
SAMUEL HAZARD,
REUBEN HAINES,
ISAIAH LUKINS,
R. E. GRIFFITH,
G. ORD,
WM. MCCLURE,
WM. MASON WALMSLEY,
T. G. LEA,
JOHN P. WETHERILL,
R. M. PATTERSON.

Dr. Lea is now in his 92d year (having been born in March of 1792) and is still young in his zeal for science and scientific work. He remarks in his recent letter, that he was informed by Professor Silliman that the reception of the list of names was the turning point with him; he thought that if a person whom he did not know personally took so much interest in the proposed publication he would have enough subscribers to insure success."—EDS.

2. *Note on the Earthquake of September 7, at Caraccas;* by A. ERNST, Professor of Natural History, University of Caraccas, (communicated in a letter dated Caraccas, May 22d, 1883.)—

In Mr. C. G. Rockwood's notes on American earthquakes, published in this Journal, vol. xxv (May), pages 353 to 360, there is under September 7 of 1882, a reference to the earthquake which visited that day the Isthmus of Panama. It is added that, "at Caraccas where the most violent shock occurred at 2.20 A. M., the loss amounted to 8 persons killed, 26 wounded, 62 buildings totally destroyed, and 67 others badly cracked." Now the truth is that *not even the slightest tremor was felt at Caraccas*, and all the reported loss, notwithstanding the apparent statistical exactitude of the report, is fortunately a mere story. It was only on the morning of the 18th of September, that we knew anything about the earthquake at Panama, by the arrival of the German steamer Allemannia at La Guaira. There were several minor shocks in August, but the "two severe earthquakes" mentioned by Mr. Rockwood, are no facts.

3. *Royal Society of New South Wales.*—The annual meeting of the Royal Society of New South Wales was held May 2. The number of new members elected during the year was 41, making the total number of ordinary members upon the roll to date, 486. At the Council meeting held on 13th December, it was unanimously resolved to award the Clarke Memorial Medal for the year 1883 to Baron Ferdinand von Mueller, K.C.M.G., F.R.S., Government Botanist, Melbourne. At the same meeting the Council awarded the prize of £25 which had been offered for the best communication upon the "influence of Australian climates and pastures upon the growth of wool" to Dr. Ross, M.L.A., Molong, and the prize for the one upon "The Aborigines of New South Wales" to Mr. John Fraser, B.A., West Maitland. During the year the Society held ten meetings at which twelve papers were read, and the medical and microscopical sections held regular monthly meetings. The sum expended upon the library during the year was £422. 12. 10.

At the annual meeting Mons. Louis Pasteur was unanimously elected an Honorary member of the Society to fill the vacancy caused by the death of the late Dr. Charles Darwin.

4. *Explorations in the region of the Gulf Stream* off the eastern coast of the United States.—An important paper on this subject, illustrated by maps and sections, by Professor A. E. Verrill, is in progress in "Science." It commenced in number 16 and is continued in number 19.

China. Ergebnisse eigener Reisen und darauf gegründeter Studien, von FERDINAND FREIHERRN VON RICHTOFEN. Vol. IV. Paleontological Part. pp. 288, with 54 plates and 15 wood-cuts. Berlin, 1883 (Dietrich Reimer).

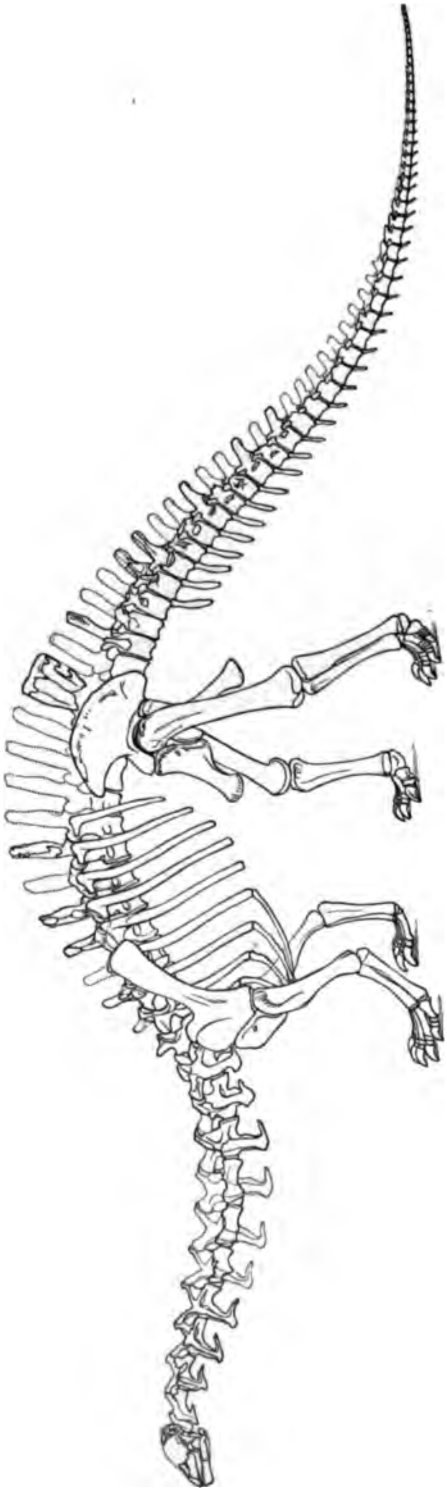
Handbuch der Klimatologie, von Dr. Julius Hann. 764 pp. 8vo. Stuttgart, 1883 (J. Engelhorn).

Electricity and Magnetism, by E. MASCART, Prof. Collège de France, and Director of the Central Metrolog. Bureau, and J. JOUBERT, Prof. Coll. Rollin; translated by E. Atkinson, Prof. Exper. Sci. in the Staff College. Vol. I, General Phenomena and Theory. 654 pp. 8vo. London, 1883 (Thos. De La Rue & Co.).

Manual of Taxidermy. A Complete Guide in Collecting and Preserving Birds and Mammals, by C. J. Maynard. 112 pp. 8vo, illustrated. Boston, 1883. (S. E. [illegible]sino & Co.).

A Visit to Ceylon, by Ernst Haeckel; translated by Clara Bell. 338 pp. 8vo. [illegible]ton, 1883 (S. E. Cassino & Co.).

AM. JOUR. SCI., Vol. XXVI, 1883. Plate



Restoration of Brontosaurus excelsus, Marsh. One-eightieth natural size.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. IX.—*Principal Characters of American Jurassic Dinosaurs.* Part VI: *Restoration of Brontosaurus*, (with plate I); by Professor O. C. MARSH.

IN the previous articles of this series, the writer has given the more important characters of the order *Sauropoda.** A volume on this group is now in preparation, and the illustrations (90 plates) are nearly completed. One of these is a restoration of *Brontosaurus*, which has so many points of interest that a reduced figure is here presented. Several new characters of this group are added, some of which will be of interest to comparative anatomists.

RESTORATION OF BRONTOSAURUS, (Plate I.)

Nearly all the bones here represented belonged to a single individual, which when alive was nearly or quite fifty feet in length. The position here given was mainly determined by a careful adjustment of these remains. That the animal at times assumed a more erect position than here represented is probable, but locomotion on the posterior limbs alone was hardly possible.

The head was remarkably small. The neck was long, and, considering its proportions, flexible, and was the lightest portion of the vertebral column. The body was quite short, and the abdominal cavity of moderate size. The legs and feet were massive, and the bones all solid. The feet were plantigrade, and each foot-print must have been about a square yard in extent. The tail was large, and nearly all the bones solid.

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*This Journal, xvi, 411, Nov. 1878; xvii, 86, Jan. 1879; xxi, 417, May, 1881; and xxiii, 81, Jan. 1882.

The diminutive head will first attract attention, as it is smaller in proportion to the body than in any vertebrate hitherto known. The entire skull is less in diameter or actual weigh than the fourth or fifth cervical vertebra.

A careful estimate of the size of *Brontosaurus*, as here restored, shows that when living the animal must have weighed more than twenty tons. The very small head and brain, and slender neural cord, indicate a stupid, slow moving reptile The beast was wholly without offensive or defensive weapons or dermal armature.

In habits, Brontosaurus was more or less amphibious, and its food was probably aquatic plants or other succulent vegetation. The remains are usually found in localities where the animals had evidently become mired.

Among the new points in the skull of the *Sauropoda* recently determined are the following:

PITUITARY FOSSA.

In *Morosaurus*, the pituitary fossa is comparatively shallow, much like that in the crocodile, and many birds, being connected with the under surface of the skull by the two usual divergent foramina for the passage of the internal carotid arteries. In *Apatosaurus*, however, it is remarkably different. Here the fossa becomes enlarged into a vertical canal, which, expanding below, communicates by a wide transverse orifice with the pharyngeal cavity. The arterial foramina are here canals thinly covered over with bone, and open just within the rim of the lower orifice. The pituitary cavity itself has a firm smooth wall throughout. The openings are both transverse, and oval in shape. The upper one is eighteen by six millimeters in its diameters; the lower opening thirty by twelve.

This remarkable connection of the cerebral cavity with the alimentary canal is an embryonic character, and corresponds to the condition observed in the chick at the fifth day of incubation. This peculiar feature appears to be a family character of the *Atlantosauridæ.*

POST-OCCIPITAL BONES.

In two genera of the *Sauropoda*, (*Morosaurus* and *Brontosaurus*), and probably in all members of this order, there is a pair of small bones connected with the skull which have not hitherto been observed in any vertebrates. These bones, which may be called the *post-occipital* bones, were found in position in one specimen, and with the skull in several others. When in place, they are attached to the occiput just above the foramen magnum, and extend backward and outward, overlapping the lateral pieces of the atlas, thus protecting the spinal cord at this point, which would otherwise be much exposed.

These bones are short, flattened, and slightly curved, resembling somewhat a riblet. The anterior end is thickened and rugose for attachment to a roughened surface on the exoccipital, just above and outside the foramen magnum. The shaft is flattened from above downward, and gradually converges to a thin posterior end. In *Morosaurus grandis*, these bones are about 65 mm. in length, and 30 along the surface which joins the occiput. They correspond in position to the muscle in mammals known as the *rectus capitis posticus minor.*

In the existing Cormorants (*Graculus*) a single slender bone is articulated to the occiput on the median line. This, however, does not correspond to the bones here described. To distinguish it from the post-occipitals, it may be called the *nuchal* bone.

STAPES WANTING.

In the skull of *Morosaurus* in which the post-occipital bones were found in position and the other bones at the base of the skull were undisturbed, a careful search was made for the stapes, but no indication of it was found. Its absence in this specimen, so well preserved, would indicate that it was wanting in this genus, if not in the other Sauropoda.

COLUMELLA PRESENT.

In a skull of *Brontosaurus* in which the bones, although displaced, were in very perfect preservation, a pair of bones were found which apparently are the columellæ. They are elongated, flattened bones, with the shaft somewhat constricted in the middle, and twisted. Their length corresponds to the elevated posterior part of the skull in this genus.

HYOID BONES.

There are two pairs of hyoid bones in the Sauropoda. They are elongated, rodlike, and somewhat curved. In *Brontosaurus excelsus*, they are 210 and 130 mm. in length respectively.

Among the other points of interest in the skull of the *Sauropoda* are the following:

The parietal bones are very short, and form but a small portion of the brain case. They are composed chiefly of the flattened arched processes, which meet with the squamosals at their outer ends. There is no parietal foramen. The squamosals lie upon the par-occipital processes. They have a short deep groove for the reception of the post-frontal. On their lower part, which descends in front of the par-occipital process, they expand into a thin spoon-shaped form, which fits over the head of the quadrate. The quadrate has an oval rounded head, and slender shaft. Below, it is firmly united to the pterygoids. On the outer side, the quadrato-jugals are attached. These bones are elongate, and slightly sigmoid in shape. The lower end is rodlike, and curved forward, descending below the articular surface of the quadrate. The pterygoids are tri-radiate bones, with the posterior ends cup-shaped, resembling the partially closed human hand. This cavity, somewhat restricted by a thumb-like process, receives the basi-pterygoid process.

THE VERTEBRÆ.

There are twenty-seven precaudal vertebræ in *Brontosaurus*, of which the first twelve bear pleurapophyses, or hatchet bones, united to the centra, and may hence be called true cervicals. Of the remaining twelve which bear free ribs, the thirteenth, fourteenth, and fifteenth have the surface for the articulation of the head of the rib on the centrum, below the neural suture.

All the precaudal vertebræ have large cavities in the centrum, communicating exteriorly with the surface by means of large lateral foramina. This cavernous structure of the vertebræ gradually decreases posteriorly, until in the anterior cauda vertebræ it is confined to a small pocket above the transverse process. The neural arches of the presacral vertebræ contai numerous deep cavities. The pleurapophyses of the cervica vertebræ are also reticulate in their structure, and some of th anterior ribs have small but deep fossæ below the tubercle.

POST-METAPOPHYSES.

On the last two or three cervical vertebræ of *Brontosauru* there is a convoluted ridge of bone over the posterior zyg pophyses. In the anterior dorsals, this ridge becomes strong and more elongated, forming a distinct protuberance. The processes have not hitherto been described. As they a analogous to the processes in mammals known as met pophyses, they may bear the same general name, being disti guished as the post-metapophyses. The term pre-metapoph ses should then be applied to the processes in mammals.

The post-metapophyses probably serve for the attachment ligaments in the place of the neural spine, which is here who wanting. These processes, which are at first oblique in po tion, gradually become more vertical and stouter, and, coalesci at their bases, finally become united throughout, and are th converted into the neural spine.

FŒTAL DINOSAURS.

Remains of a very small dinosaur were found in immediate relation with the type specimen of *Morosaurus grandis*. These remains, which consist of a complete femur, the larger portion of both humeri, and several vertebræ, show no essential differences from the large specimens except in size, and indicate an animal of perhaps seven feet in length, and little more than two feet in height. The imperfect ossification of these bones indicates that the animal was very young, and it seems probable that it was fœtal. The only other similar case known in the *Dinosauria* is the apparent embryo observed by the writer in *Compsognathus*.*

CLASSIFICATION.

The various genera of the *Sauropoda*, and in fact of the *Dinosauria* in general, cannot at present be distinguished by the detached teeth. In one form, however, the teeth are quite peculiar, and the dentition appears to offer generic characters. The maxillary teeth of this form have been referred to *Stegosaurus* (this Journal, vol. xix, p. 255; pl. VI, figs 4 and 5), as they were first found in connection with the remains of that genus. Later investigations indicate that they belong to the *Sauropoda*, and there is some evidence that they are the teeth of *Diplodocus*.

The main characters of the order *Sauropoda*, and of the two families now known to belong to it, are as follows:

Order SAUROPODA. Herbivorous.

Feet plantigrade, ungulate; five digits in manus and pes; second row of carpal and tarsal bones unossified. Pubes projecting in front, and united distally by cartilage; no post-pubis. Fore and hind limbs nearly equal; limb bones solid. Sternal bones parial. Premaxillary bones with teeth. Precaudal vertebræ hollow. Each sacral vertebra supports its own transverse processes.

Family *Atlantosauridæ*. Anterior vertebræ opisthocœlian. Ischia directed downward, with extremities meeting on median line. Anterior caudals with lateral cavities. A pituitary canal.

Family *Morosauridæ*. Anterior vertebræ opisthocœlian. Ischia directed backward, with sides meeting on median line. Anterior caudals solid. Pituitary fossa only.

Yale College, New Haven, July 12th, 1883.

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* This Journal, xxii, 340, November, 1881.

ART. X.—*The Evolution of the American Trotting-Horse;* by FRANCIS E. NIPHER.

SINCE my paper in the July number of this Journal was written, I have calculated the constants in the differential equation by mathematical methods, and have obtained a result differing slightly from that given in the July number. The most probable value for the minimum time of trotting a mile turns out to be 91 seconds, instead of 93 seconds as was obtained by graphical methods.

The final equation is

$$\log (s-91) = 1{\cdot}703-0{\cdot}0046\,T,$$

where s is the time (in seconds) of trotting a mile, and T is estimated in years from 1860.

This equation does not give essentially different results from the former one, the only point of interest being in the new value for the limiting time. The probable error of this value is not over four seconds, and it is not likely that the running horse will beat his present record by five seconds, so that it is very probable that the trotter will finally surpass the running horse.

This conclusion does not rest solely on mathematical evidence. The trotter carries his body more steadily—with less of rise and fall—than the runner, and it seems very reasonable that this should result to the advantage of the trotter, when the process of developing and adjusting his muscles and chest shall have been sufficiently carried on, so that the contest between the two animals shall have been reduced to a matter of muscular capacity.

It is well known that some herds of wild horses on the Texas plains were natural pacers, and even when pushed to the utmost and for days together by the best running horses, they nearly all held their gait. One large white pacer became widely known and many attempts were made to capture him, but he proved more than a match for the best horses that could be brought against him.

The record of the trotting-horse during the next ten years will enable us to determine the limiting speed with sufficient precision to settle the matter beyond question. It is even now evident that our good ancestors, who thought it wicked to run horses, builded more wisely than they knew when they began the evolution of the American trotting-horse.

Art. XI.—*On Concave Gratings for Optical Purposes;** by Henry A. Rowland, Professor of Physics, Johns Hopkins University, Baltimore.

General Theory.

Having recently completed a very successful machine for ruling gratings, my attention was naturally called to the effect of irregularity in the form and position of the lines and the form of the surface on the definition of the grating. Mr. C. S. Peirce has recently shown, in the American Journal of Mathematics, that a periodic error in the ruling produces what have been called ghosts in the spectrum. At first I attempted to calculate the effect of other irregularities by the ordinary method of integration, but the results obtained were not commensurate with the labor. I then sought for a simpler method. Guided by the fact that inverse methods in electrical distribution are simpler than direct methods, I soon found an inverse method for use in this problem.

In the use of the grating in most ordinary spectroscopes, the telescopes are fixed together as nearly parallel as possible, and the grating turned around a vertical axis to bring the different spectra into the field of view. The rays striking on the grating are nearly parallel, but for the sake of generality I shall assume that they radiate from a point in space and shall investigate the proper ruling of the grating to bring the rays back to the point from which they started. The wave fronts will be a series of spherical shells at equal distances apart. If these waves strike on a reflecting surface, they will be reflected back provided they can do so all in the same phase. A sphere around the radiant point satisfies the condition for waves of all lengths and thus gives the case of ordinary reflection. Let any surface cut the wave surfaces in any manner and let us remove those portions of the surface which are cut by the wave surfaces: the light of that particular wave-length can then be reflected back along the same path in the same phase and thus, by the above principle, a portion will be sent back. But the solution holds for only one wave-length and so white light will be drawn out into a spectrum. Hence we have the important conclusion that a theoretically perfect grating for one position of the slit and eyepiece can be ruled on *any* surface, flat or otherwise. This is an extremely important practical conclusion and explains many facts which have been observed in the use of gratings. For we see that errors of the dividing engine can be counterbalanced by errors in the flatness of the plate, so that a bad dividing engine may now and then make a grating which is good in one spectrum but not in all. And so we often find that one spectrum is better than another. Furthermore Professor Young has observed that he could often improve the definition of a grating by slightly bending the plate on which it was ruled.

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* An abstract of this paper with some other matter was given at the Physical Society of London in November last, the paper being in my hand in its present shape at that time. As I wished to make some additions, for which I have not yet had time, I did not then publish it. I was much surprised soon after to see an article on this subject, which had been presented to the Physical Society and was published in the Philosophical Magazine. The article contains nothing more than an extension of my remarks at the Physical Society and formulæ similar to those in this paper. As I have not before this published anything except a preliminary notice of the concave gratings, I expected a little time to work up the subject, seeing that the practical work of photographing the spectrum has recently absorbed all my time. But probably I have waited too long.

From the above theorem we see that if a plate is ruled in circles whose radius is $r \sin \mu$ and whose distance apart is $\Delta r \sin \mu$, where Δr is constant, then the ruling will be appropriate to bring the spectrum to a focus at a distance, r, and angle of incidence, μ. Thus we should need no telescopes to view the spectrum in that particular position of the grating. Had the wave surfaces been cylindrical instead of spherical the lines would have been straight instead of circular, but at the above distances apart. In this case the spectrum would have been brought to a focus, but would have been diffused in the direction of the lines. In the same way we can conclude that in flat gratings any departure from a straight line has the effect of causing the dust in the slit and the spectrum to have different foci, a fact sometimes observed.

We also see that, if the departure from equal spaces is small, or, in other words, the distance r is great, the lines must be ruled at distances apart represented by

$$c\left(1-\frac{\cos^2 \mu}{r \sin \mu}x+\&c.\right)$$

in order to bring the light to a focus at the angle μ and distance r, c being a constant and x the distance from some point on the plate. If μ changes sign, then r must change in sign. Hence we see that the effect of a linear error in the spacing is to make the focus on one side shorter and the other side longer than the normal amount. Professor Peirce has measured some of Mr. Rutherfurd's gratings and found that the spaces increased in passing along the grating, and he also found that the foci of symmetrical spectra were different. But this is the first attempt to connect the two. The definition of a grating may thus be very good even when the error of run of the screw is considerable, provided it is linear.

CONCAVE GRATINGS.

Let us now take the special case of lines ruled on a spherical surface. And let us not confine ourselves to light coming back to the same point, but let the light return to another point, let the coördinates of the radiant point and focal point be $y=0$, $x=-a$ and $y=0$, $x=+a$, and let the center of the sphere whose radius is ρ be at x', y'. Let r be the distance from the radiant point to the point x, y, and let R be that from the focal point to x, y. Let us then write

$$2b=\mathrm{R}+rc.$$

Where c is equal to ± 1 according as the reflected or transmitted ray is used. Should we increase b by equal quantities and draw the ellipsoids or hyperboloids so indicated, we could use these surfaces in the same way as the wave surfaces above. The intersections of these surfaces with any other surface form what are known as Huyghen's zones. By actually drawing these zones on the surface, we form a grating which will diffract the light of a certain wave-length to the given focal point. For the particular problem in hand, we need only work in the plane x, y for the present.

Let s be an element of the curve of intersection of the given surface with the plane x, y. Then our present problem is to find the width of Huyghen's zones on the surface, that is ds in terms of db.

The equation of the circle is

$$(x-x')^2+(y-y')^2=\rho^2$$

and of the ellipse or hyperbola

$$\mathrm{R}+rc=2b$$

or

$$(b^2-a^2)\,x^2+b^2y^2=b^2\,(b^2-a^2)$$

in which c has disappeared

$$ds=\sqrt{dx^2+dy^2}\,; \quad \frac{dx}{dy}=-\frac{y-y'}{x-x'}$$

$$dx\left\{ (b^2-a^2)\,x-b^2y\frac{x-x'}{y-y'} \right\}=b\{2b^2-(x^2+y^2+a^2)\}db$$

$$dy\left\{ -(b^2-a^2)x\frac{y-y'}{x-x'}+b^2y \right\}=b\{2b^2-(x^2+y^2+a^2)\}db$$

$$\therefore \frac{ds}{db}=\rho b\frac{2b^2-(x^2+y^2+a^2)}{(b^2-a^2)\,(y-y')\,x-b^2\,(x-x')y}$$

This equation gives us the proper distance of the rulings on the surface, and if we could get a dividing engine to rule according to this formula the problem of bringing the spectrum to a focus without telescopes would be solved. But an ordinary dividing engine rules equal spaces and so we shall further investigate the question whether there is any part of the circle where the spaces are equal. We can then write

$$\frac{ds}{db}=\mathrm{C}.$$

And the differential of this with regard to an arc of the circle must be zero. Differentiating and reducing by the equations

$$\frac{dx}{dy}=-\frac{y-y'}{x-x'};\quad \frac{db}{dy}=-\frac{\rho}{\mathrm{C}\,(x-x')}$$

we have

$$\rho\left\{2xb\,(y-y')-2yb\,(x-x')-\frac{\rho}{\mathrm{C}}\,[6b^2-(x^2+y^2+a^2)]\right\}$$

$$+\mathrm{C}\left\{(y-y')[(b^2-a^2)(y-y')-b^2y]-(x-x')[(b^2-a^2)x-b^2(x-x')]\right.$$

$$\left.+\frac{2b\rho}{\mathrm{C}}\,[x\,(y-y')-y\,(x-x')]\right\}=0.$$

It is more simple to express this result in terms of R, r, ρ and the angles between them.

Let μ be the angle between ρ and r, and ν that between ρ and R. Let us also put

$$\alpha=\frac{\mu-\nu}{2}\quad\text{and}\quad \varepsilon=\frac{\mu+\nu}{2}.$$

Let β, γ and δ also represent the angles made by r, R and ρ respectively with the line joining the source of light and focus, and let

$$\eta=\frac{\beta+\gamma}{2}.$$

Then we have

$$x=\frac{\mathrm{R}\cos\gamma+r\cos\beta}{2};\quad y=\frac{\mathrm{R}\sin\gamma+r\sin\beta}{2};\quad a=\frac{r\cos\beta-\mathrm{R}\cos\gamma}{2}$$

$$(b^2-a^2)\,(y-y')^2+b^2(x-x')^2=\rho^2(b^2-a^2\sin^2\delta)$$

$$b^2-a^2=\mathrm{R}r\cos^2\alpha$$

$$\sin\eta=\frac{\mathrm{R}+r}{2a}\sin\alpha;\quad \cos\eta=\frac{r-\mathrm{R}}{2a}\cos\alpha$$

$$\mathrm{R}=b-\frac{a}{b}x;\quad r=b+\frac{a}{b}x$$

$$x=b\,\frac{\cos\eta}{\cos\alpha};\quad y=a\,\frac{\sin\gamma\,\sin\beta}{\sin\alpha\,\cos\alpha}=\frac{\mathrm{R}r}{b}\sin\eta\cos\alpha$$

$$b^2y\,(y-y')+x\,(b^2-a^2)\,(x-x')=\frac{b\mathrm{R}r\rho}{2}\,(\cos\mu+\cos\nu)$$

$$2b^2-(x^2+y^2+a^2)=\mathrm{R}r$$

$$x(b^2-a^2)(y-y')-b^2y(x-x')=\frac{Rrb\rho}{2}(\sin\mu+\sin\nu)$$

$$C=\frac{2}{\sin\mu+\sin\nu}=\frac{1}{\cos\alpha\sin\varepsilon}$$

$$2a\cos\delta=r\cos\mu-R\cos\nu$$

$$2a\sin\delta=r\sin\mu-R\sin\nu.$$

On substituting these values and reducing, we find

$$\rho^{*}=\frac{2Rr\cos\alpha\cos\varepsilon}{r\cos^2\nu+R\cos^2\mu}.$$

Whence the focal length is

$$r=\frac{\rho R\cos^2\mu}{2R\cos\alpha\cos\varepsilon-\rho\cos^2\nu}.$$

For the transmitted beam, change the sign of R. Supposing ρ, R and ν to remain constant and r and μ to vary, this equation will then give the line on which all the spectra and the central image are brought to a focus.

By far the most interesting case is obtained by making

$$r=\rho\cos\mu\quad R=\rho\cos\nu,$$

since these values satisfy the equation. The line of foci is then a circle with a radius equal to one-half ρ. Hence if a

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[*] A more simple solution is the following; $\frac{ds}{db}$ must be constant in the direction in which the dividing engine rules. If the dividing engine rules in the direction of the axis, y, the differential of this with respect to y must be zero. But we can also take the reciprocal of this quantity and so we can write for the equation of condition

$$\frac{d}{dy}\,\frac{d(R+r)}{ds}=0.$$

Taking a circle as our curve we can write

$$(x-x')^2+(y-y')^2=\rho^2$$

and

$$(x-x'')^2+(y-y'')^2=R^2$$

$$(x-x''')^2+(y-y''')^2=r^2$$

$$\frac{d(R+r)}{ds}=\frac{1}{\rho}\left\{(y-y')\left(\frac{x-x''}{R}+\frac{x-x'''}{r}\right)+(x-x')\left(\frac{y-y''}{R}+\frac{y-y'''}{r}\right)\right\}$$

$$\frac{d}{dy}\,\frac{d(R+r)}{ds}=\frac{1}{\rho}\left\{\frac{x-x''}{R}+\frac{x-x'''}{r}-(y-y')\left[\frac{(x-x'')(y-y'')}{R^3}+\frac{(x-x''')(y-y''')}{r^3}\right]\right.$$

$$\left.+(x-x')\left[\frac{(x-x'')^2}{R^3}+\frac{(x-x''')^2}{r^3}\right]\right\}=0.$$

Making

$$x=0,\ y=0,\ y'=0,\ x'=\rho$$

we have

$$\frac{x''}{R}+\frac{x'''}{r}-\rho\left(\frac{x''^2}{R^3}+\frac{x'''^2}{r^3}\right)=0,$$

or

$$\rho=Rr\frac{\cos\mu+\cos\nu}{r\cos^2\nu+R\cos^2\mu}=\frac{2Rr\cos\alpha\cos\varepsilon}{r\cos^2\nu+R\cos^2\mu}.$$

source of light exist on this circle, the reflected image and all the spectra will be brought to a focus on the same circle. Thus if we attach the slit, the eye-piece and the grating to the three radii of the circle, however we move them, we shall always have some spectrum in the focus of the eye-piece. But in some positions the line of foci is so oblique to the direction of the light that only one line of the spectrum can be seen well at any one time. The best position of the eye-piece as far as we consider this fact is thus the one opposite to the grating and at its center of curvature. In this position the line of foci is perpendicular to the direction of the light, and we shall show presently that the spectrum is normal at this point whatever the position of the slit, provided it is on the circle.

1.

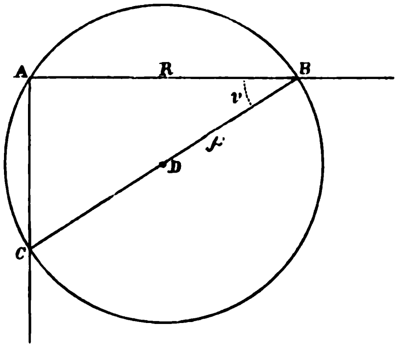

Fig. 1 represents this case; A is the slit, C is the eye-piece, and B is the grating with its center of curvature at C. In this case all the conditions are satisfied by fixing the grating and eye-piece to the bar BC whose ends rest on carriages moving on the rails AB and AC at right angles to each other; when desired, the radius AD may be put in to hold everything steady, but this has been found practically unnecessary.

The proper formulæ for this case are as follows. If λ is the wave-length and w the distance apart of the lines of the grating from center to center, then we have

$$\frac{1}{C}=\frac{\lambda N}{2w}=\frac{\sin\nu}{2}$$

where N is the order of the spectrum.

$$\therefore\ \lambda=\frac{w\sin\nu}{N}.$$

Now in the given case ρ is constant and so $N\lambda$ is proportional to the line AC. Or, for any given spectrum, the wave-length is proportional to that line.

If a micrometer is fixed at C we can consider the case as follows:

$$\frac{1}{C}=\frac{\lambda N}{2w}=\frac{1}{2}(\sin\mu+\sin\nu)$$

$$\frac{d\lambda}{d\mu}=\frac{w}{N}\cos\mu.$$

If D is the distance the cross-hairs of the micrometer move forward for one division of the head, we can write for the point C

$$d\mu=\frac{D}{\rho}$$

and for the same point μ is zero. Hence

$$\varDelta\lambda=\frac{wD}{N\rho}.$$

But this is independent of ν and we thus arrive at the important fact that the value of a division of the micrometer is always the same for the same spectrum and can always be determined with sufficient accuracy from the dimensions of the apparatus and number of lines on the grating, as well as by observation of the spectrum.

Furthermore, this proves that the spectrum is normal at this point and to the same scale in the same spectrum. Hence we have only to photograph the spectrum to obtain the normal spectrum and a centimeter for any of the photographs always represents the same increase of wave-length.

It is to be specially noted that this theorem is *rigidly true* whether the adjustments are correct or not, provided only that the micrometer is on the line drawn perpendicularly from the center of the grating, even if it is not at the center of curvature.

As the radius of curvature of concave gratings is usually great, the distance through which the spectrum remains practically normal is very great. In the instrument which I principally use, the radius of curvature ρ, is about 21 feet 4 inches, the width of the ruling being about 5·5 inch. In such an instrument the spectrum thrown on a flat plate is normal within about 1 part in 1,000,000, for 6 inches and less than 1 in 35,000, for 18 inches. In photographing the spectrum on a flat plate, the definition is excellent for 12 inches, and by use of a plate bent to 11 feet radius, a plate of 20 inches in length is in perfect focus and the spectrum still so nearly normal as to have its error neglected for most purposes.

Another important property of the concave grating is that all the superimposed spectra are in focus at the same point, and so by micrometric measurements the relative wave-lengths are readily determined. Hence, knowing the absolute wave-length of one line, the whole spectrum can be measured. Professor Peirce has determined the absolute wave-length of one line with great care and I am now measuring the coincidences. This method is greatly more accurate than any hitherto known, as by a mere eye inspection, the relative wave-length can often be judged to 1 part in 20,000 and with a micrometer to 1 in 1,000,000. Again, in dealing with the invisible portion of the spectrum, the focus can be obtained by examining the superimposed spectrum. Captain Abney, by using a concave mirror in the place of telescopes, has been enabled to use this method for obtaining the focus in photographing the ultra red rays of the spectrum. It is also to be noted that this theorem of the normal spectrum applies also to the flat grating used with telescopes and to either reflecting or transmitting gratings; but in these cases only a small portion of the spectrum can be used as no lens can be made perfectly achromatic. And so, as the distance of the micrometer has constantly to be changed when one passes along the spectrum, its constant does not remain constant but varies in an irregular manner. But it would be possible to fix the grating, one objective and the camera rigidly on a bar, and then focus by moving the slit or the other objective. In this case the spectrum would be rigidly normal, but would probably be in focus for only a small length and the adjustment of the focus would not be automatic.

But nothing can exceed the beauty and simplicity of the concave grating when mounted on a movable bar such as I have described and illustrated in Fig. 1. Having selected the grating which we wish to use, we mount it in its plate-holder and put the proper collimating eye-piece in place. We then carefully adjust the focus by altering the length of D until the cross-hairs are at the exact center of curvature of the grating. On moving the bar the whole series of spectra are then in exact focus, and the value of a division of the micrometer is a known quantity for that particular grating. The wooden way AC, on which the carriage moves is graduated to equal divisions representing wave-lengths, since the wave-length is proportional to the distance AC. We can thus set the instrument to any particular wave-length we may wish to study, or even determine the wave-length to at least one part in five thousand by a simple reading. By having a variety of scales, one for each spectrum, we can immediately see what lines are superimposed on each other and identify them accordingly when we are measuring their relative wave-length. On now replacing the eye-piece by a camera, we are in a position to photograph the spectrum with the greatest ease. We put in the sensitive plate, either wet or dry, and move to the part we wish to photograph; having exposed for that part, we move to another part, raise the plate to another position and expose once more. We have no thought for the focus, for that remains perfect, but simply refer to the table giving the proper exposure for that portion of the spectrum and so have a perfect plate. Thus we can photograph the whole spectrum on one plate in a few minutes, from the F line to the extreme violet in several strips, each 20 inches long. Or we may photograph to the red rays by prolonged exposure. Thus the work of days with any other apparatus becomes the work of hours with this. Furthermore each plate is to scale, an inch on any one of the strips representing *exactly* so much difference of wave-length. The scale of the different orders of spectra are *exactly* proportional to the order. Of course the superposition of the spectra gives the relative wave-length. To get the superposition, of course, photography is the best method.

Having so far obtained only the first approximation to the theory of the concave grating, let us now proceed to a second one. The dividing engine rules equal spaces along the chord of the circular arc of the grating: the question is whether any other kind of ruling would be better. For the dividing engine is so constructed that one might readily change it to rule slightly different from equal spaces.

The condition for theoretical perfection is that C shall remain constant for all portions of the mirror. I shall therefore investigate how nearly this is true.

Let ρ be the radius of curvature and let R and r be the true distances to any point of the greating, R_0 and r_0 being the distances to the center. Let μ and ν be the general values of the angles and μ_0 and ν_0 the angles referred to the center of the mirror. The condition is that

$$\frac{2}{C}=\sin\mu+\sin\nu$$

shall be a constant for all parts of the surface of the grating. Let us then develop $\sin\mu$ and $\sin\nu$ in terms of μ_0, ν_0 and the angle δ between the radii drawn to the center of the grating and to the point under consideration. Let δ' be the angle between R and R_0. Then we can write immediately

$$\rho\sin\mu=\rho\sin\mu_0\cos\delta'+R_0\sin\delta'-\rho\cos\mu_0\sin\delta'$$

$$\sin\mu=\sin\mu_0\cos\delta'\left\{1+\frac{R_0}{\rho\sin\mu_0}A\tan\delta'\right\}$$

where

$$A=1-\frac{\rho\cos\mu_0}{R_0}.$$

Developing the value of $\cos\delta'$ in terms of δ, we have

$$\cos\delta'=\cos\delta\left\{1+\frac{A}{2}\left[1+\frac{\rho\cos\mu_0}{R_0}\right]\delta^2-\right.$$

$$\left.\frac{\rho\sin\mu_0}{2R_0}\left[1+A\left(1+\frac{2\rho}{R_0}\right)\right]\delta^3+\&$$

As the cases we are to consider are those where A is sm it will be sufficient to write

$$\tan\delta'=\frac{\rho\cos\mu_0}{R_0}\delta.$$

Whence we have

$$\sin\mu=\sin\mu_0\cos\delta\left\{1+\cot\mu_0 A\delta+\frac{A}{2}\left[1+\frac{\rho\cos\mu_0}{R_0}\right]\delta^2\right.$$

$$+\left[A^2\cot\mu_0\left(1+\frac{\rho\cos\mu_0}{R_0}\right)-\frac{\rho\sin\mu_0}{2R_0}\left(1+A\left(1+\frac{2\rho}{R_0}\right)\right)\right]\delta^3+\&c$$

We can write the value of $\sin\nu$ from symmetry. But we h

$$2\frac{db}{ds}=\sin\mu+\sin\nu.$$

In this formula, *db* can be considered as a constant depend on the wave-length of light, etc., and *ds* as the width apart the lines on the grating. The dividing engine rules lines the curved surface according to the formula

$$2\frac{db}{ds}=\cos\delta(\sin\mu_0+\sin\nu_0).$$

But this is the second approximation to the true theoreti ruling. And this ruling will not only be approximately c rect, but exact when all the terms of the series except the fi vanish. In the case where the slit and focus are on the cir of radius $\frac{1}{2}\rho$, as in the automatic arrangement described abo we have $A=0$ and the second and third terms of the ser disappear, and we can write since we have

$$\frac{R_0}{\rho}=\cos\mu_0 \text{ and } \frac{r_0}{\rho}=\cos\nu_0$$

$$2\frac{db}{ds}=\cos\delta(\sin\mu_0+\sin\nu_0)\left(1-\frac{1}{2}\,\frac{\sin\mu_0\tan\mu_0+\sin\nu_0\tan\nu_0}{\sin\mu_0+\sin\nu_0}\delta^2+\&\right.$$

But in the automatic arrangement we also have $\nu_0=0$, and the formula becomes

$$2\frac{db}{ds}=\cos\delta(\sin\mu_0+\sin\nu_0)\left\{1-\frac{1}{2}\tan\mu_0\,\delta^2+\&c.\right\}$$

find the greatest departure from theoretical perfection refer to the edge of the grating. In the gratings whi

I am now making, ρ is about 260 inches and the width of the grating about 5·4 inches. Hence $\delta=\frac{1}{100}$ approximately and the series becomes

$$1-\frac{1}{2,000,000}\tan\mu_0.$$

Hence the *greatest* departure from the theoretical ruling, even when $\tan\mu_0=2$, is 1 in 1,000,000. Now the distance apart of the components of the 1474 line is somewhat nearly one forty-thousandth of the wave-length and I scarcely suppose that any line has been divided by the best spectroscope in the world whose components are less than one-third of this distance apart. Hence we see that the departure of the ruling from theoretical perfection is of little consequence until we are able to divide lines twenty times as fine as the 1474 line. Even in that case, since the error of ruling varies as δ^2, the greater portion of the grating would be ruled correctly.

The question now comes up as to whether there is any limit to the resolving power of a spectroscope. This evidently depends upon the magnifying power and the apparent width of the lines. The magnifying power can be varied at pleasure and so we have only to consider the width of the lines of the spectrum. The width of the line evidently depends, in a perfect grating, upon three circumstances, the width of the slit, the number of lines in the grating and the *true physical width of the line.* The width of the slit can be varied at pleasure, the number of lines on the grating can be made very great (160,000 in one of mine), and hence we are only limited by the true physical width of the lines. We have numerous cases of wide lines, such as the C line, the components of the D* and H lines and numerous others which are perfectly familiar to every spectroscopist. Hence we are free to suppose that all lines have some physical width, and we are limited by that width in the resolving power of our spectroscope. Indeed, from a theoretical standpoint, we should suppose this to be true: for the molecules only vibrate freely while swinging through their free path and in order to have the physical width one one-hundred-thousandth of the wave-length, the molecule must make somewhat nearly one hundred thousand vibrations in its free path: but this would require a free path of about two inches! Hence it would be only the outermost solar atmosphere that could produce such fine lines and we could hardly expect to see much finer ones in the solar spectrum. Again* it is found impossible to obtain interference between two rays whose paths differ by much more than 50,000 wave-lengths.

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* I have recently discovered that each component of the D line is double probably from the partial reversal of the line as we nearly always see it in the flame spectrum.

All the methods of determining the limits seem to point to about the 150,000th of the wave-length as the smallest distance at which the two lines can be separated in the solar spectrum by even a spectroscope of infinite power. As we can now nearly approach this limit I am strongly of the opinion that we have nearly reached the limit of resolving power, and that we can never hope to see very many more lines in the spectrum than can be seen at present, either by means of prisms or gratings.

It is not to be supposed, however, that the average wave-length of the line is not more definite than this, for we can easily point the cross hairs to the center of the line to perhaps 1 in 1,000,000 of the wave-length. The most exact method of detecting the coincidences of a line of a metal with one in the solar spectrum would thus be to take micrometric measurements first on one and then on the other; but I suppose it would take several readings to make the determination to 1 in 1,000,000.

Since writing the above I have greatly improved my apparatus and can now photograph 150 lines between the H and K lines, including many whose wave-length does not differ more than 1 in about 80,000. I have also photographed the 1474 and b_3 and b_4, widely double, and also E just perceptibly double. With the eye much more can be seen, but I must say that I have not yet seen many signs of reaching a limit. The lines yet appear as fine and sharp as with a lower power. If my grating is assumed to be perfect, in the third spectrum I should be able to divide lines whose wave-lengths differed, 1 in about 150,000, though not to photograph them.

The E line has components, about $\frac{1}{60000}$th of the wave-length apart. I believe I can resolve lines much closer than this, say 1 in 100,000 at least. Hence the idea of a limit has not yet been proved.

However as some of the lines of the spectrum are much wider than others we should not expect any *definite* limit, but a gradual falling off as we increase our power. At first, in the short wave-lengths at least, the number of lines is nearly proportional to the resolving power, but this law should fail as we approached the limit.

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* This method of determining the limit has been suggested to me by Prof. C. S. Hastings, of this University.

ART. XII.—*Glacial Markings of Unusual Forms in the Laurentian Hills*;* by EDMUND ANDREWS, M.D., LL.D.

TWO summer vacations spent in camps and canoes where the Laurentian Hills skirt the northeast shore of Lake Huron have brought to my notice some glacial phenomena of very unusual forms.

These hills are for the most part ranges of hard metamorphic rock, which, at the bowlder drift period, were scraped bare and washed clean of all earthy covering, so that the present forests grow mostly in a thin stratum of vegetable mold of later origin. The irregular and knobby eminences, as the country slopes gradually beneath the level of Lake Huron, project their countless summits above the water in a wide belt of wooded islands, which extend along the coast some two hundred and fifty miles. The British chart (Bayfield's original) marked fifty-two thousand of these islands. The rocks along the coast are mostly white quartzite and gneiss, and are everywhere covered with glacial markings, which are often of peculiar forms. North and northeast of Grand Manitoulin Island, the Cloche Mountains stretch east and west about thirty miles along the coast. These mountains are of white quartzite and the strata are nearly perpendicular, with their striæ parallel to the range, that is, east and west. They are covered everywhere with striations, which, owing to the intense hardness of the material, retain their forms with beautiful distinctness.

CURVED STRIÆ.

Among the interesting phenomena of the region are the thousands of curved striations. Fig. 1 is from the stream that comes down by the Hudson's Bay Post, about fifteen rods above the cascade. There is a broad gap here in the Cloche Range through which some fine lakes, lying behind the mountains, send their surplus waters to Lake Huron. The stream apparently ran in the same bed before the drift period, for the little gorge at D (indicated in the figure by shading), scarcely ten feet deep, is well scored to the very bottom with glacial striæ following all the sinuosities of the stream. .The general course of the striæ of the gap is southward. The perpendicular lines in the figure indicate this general direction, while the curved lines in the shading of the gorge D show how the striæ bend abruptly in order to follow around the hard quartzite banks of the stream, and in doing so even run a little contrary to the main course of the glaciation.

The crest of the Cloche Mountains is crossed by a multitude of giant grooves, some of which reach a depth of six feet, are twenty feet across. These markings run southward, rou

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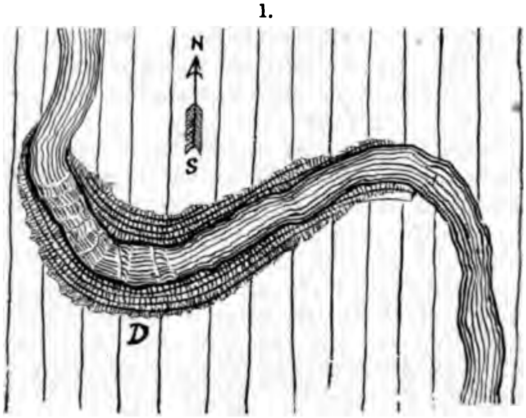
* Read before the Chicago Academy of Sciences.

1.



ing slightly over the summit of the range and down its slo until they reach the crests of its southern precipices where t terminate abruptly, as it were, sailing away into the air not forming any grooves down its face.

Sixty miles southeast of the Cloche gap and off the mo of French River, there lies, outside of the general insular l a beautiful cluster of wooded islets, the Bustard Isles. group consists of about two hundred great *roches mouton* upon which sufficient vegetable mold has accumulated to s port a thick growth of trees. Wherever this soil has b washed away by the waves the striæ come to view.

2.

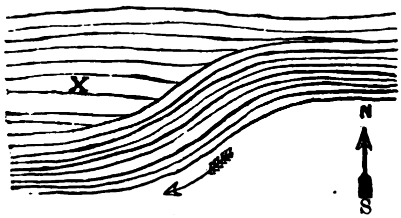
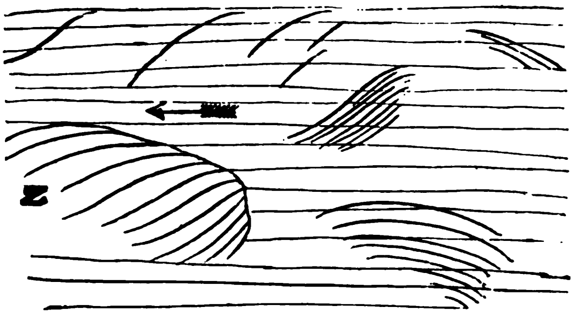


Fig. 2 was sketched f a sample of curved marki near one of my camps the north side of the gro The sketch represents ab fifteen feet of the length the marks. It is not e to see any reason for curve, as there was no pr inence of rock in the di tion of X to turn the ice. In fact the islet was highest on side toward which the ice turned at the first curve. compass mark is approximately, but not precisely correct.

Fig. 3 is copied from my notes of observation on striæ fo at Negaunee, in northern Michigan. A knob of rock unc ered by iron miners was of such material that it showed on irregular surface the finest markings, even to hair lines. Th were upon this rock some curves which were evidently deflections caused by knobs and bosses on its surface, as for instance

3.

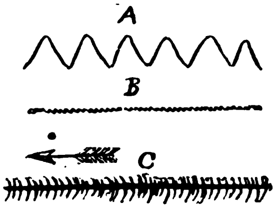


at Z. While other markings were erratic and curved without obvious cause, as though the ice had been swayed by swirling currents as the waters moved about it. The most of the glaciation was in the direction shown by the horizontal lines. The curved lines in the figure were selected from hundreds of others on a surface of about two rods square. They were generally short, and some of the curves were of less than one foot radius.

SERRATED STRIÆ.

Behind the Cloche Mountains the Spanish River runs westward into Lake Huron. A branch of this stream, called the Sable, coming down from the hills on the north, presents near its mouth five cataracts within a distance of eight miles. At the lowest of the falls the river runs through a sort of rock flume, having upon both sides walls about forty feet in height, not quite vertical but with a slight inclination away from the stream. These cliffs are smooth and striated in every part parallel to the stream. At the falls, which are only a few feet in height, the striæ curve with the descent, and also laterally with a bend of the cliff. On the walls of the gorge are to be seen a few examples of the marks A and B. Fig. 4 and the mark A is serrated, the serrations being perhaps twelve inches high. It is not easy to explain the cause of these striæ in a perfectly satisfactory manner, but it would seem that ice must have been driven through the flume with a rocking motion so that the bowlders on its lateral margins were caused to take a zigzag course, scoring the walls in a corresponding form. In B, fig. 4,

4.



is represented a section of certain marks produced in the same manner as those cuts of a stone planing engine where the tool trembles or vibrates in the grasp of the machine so as to cut a finely serrated groove. In the specimens found the serrations were about one-quarter of a centimeter from crest to crest. It is possible that the regular vibrations thus recorded on the rocks had some fixed mathematical relation to the velocity of the ice, which might possibly be determined by calculation or experiment.

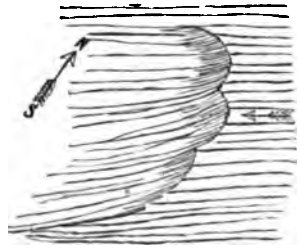
New Index of the Direction of Motion.

The mark C, fig. 4, was copied from a granitic *roche moutoneé* at the fishing hamlet of Killarney near some quartzite ranges named "Killarney Mountains." Striæ of this type show a multitude of minute cracks extending laterally and curved so as to present their points forward in the direction of the glacial movements. Only large grooves and scratches show this peculiarity. It is a very convenient mode of determining the direction of the motion, for the pressure of the bowlders which made the scratches as they moved forward caused the transverse cracks to present their concave sides always forward. It is well known to engineers that in brittle substances the forms assumed by fractures vary with the velocity of the impact, so that there is probably a time relation involved here also which would make it possible to determine the approximate velocity of the ice which carried the bowlders. Such a calculation, if based upon carefully repeated experiments, might prove an important contribution to our scanty knowledge of the Drift Period.

Scoop Marks.

These are singular phenomena and very difficult of explanation. They are of two varieties, the striated and the unstriated. Fig. 5 is a diagram intended to illustrate a typical form of the striated variety. The marks consist of shallow elongated excavations or troughs which may be many feet in width. They run nearly in the direction of the general striation of the locality, and look as though a huge flour scoop had been inserted into the rock and had cut out enough of its substance to make a smooth and rather shallow concave channel. The end toward the east, or northeast from whence the drift action came, is abrupt, sharply-defined, and although the angle of junction with

5.



the general surface is obtuse, yet this line of junction is a sharp and clearly cut edge and not a gradual sloping or rounding into the general level. The interior of the concavity is well striated in the direction of its own length, but the scratches are never continuous with those of the general surface northeast of the head of the scoop mark ; and, conversely, the striæ of the general glaciated surface are all cut off abruptly at the head of the scoop and never descend at all into it.

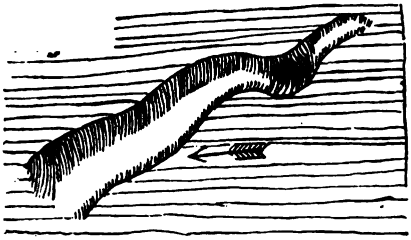
The tail of the channel grows shallower as it runs west or southwestward, and its thin extremity merges imperceptibly into the glaciated surrounding surface. A few of the scoop-marks are nearly flat in contour and are only recognizable where they cut abruptly through at an obtuse angle into the general face of the rock.

The interpretation of these anomalous scoop-marks is somewhat perplexing; at least, after considerable study of them, I am quite unable to frame a plausible theory of their origin other than the general one, that ice was the agent.

The unstriated scoop-marks differ considerably from the others in form, and on the average are smaller, seldom exceeding two feet in breadth. There are no glacial markings in their smooth but unpolished concavities.

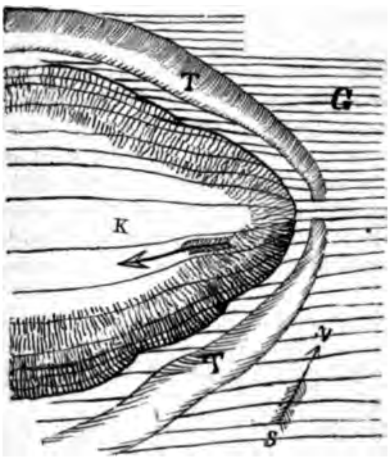
Whether they never contained any striations or whether the markings have been erased by sand and water driven through them before the Drift Period closed is not easily determined. These channels generally are crooked without obvious reason (see fig. 6), and are narrowest at the end toward the northeast, from which direction the drift came. Toward the opposite extremity they grow wider and more shallow, disappearing finally and vaguely into the general ice-marked surface of the locality. The specimen shown in fig. 6 is perhaps six feet long, sunk in granite, without any crevice-vein or visible irregularity in the rock face to account for its excavation at that spot. These channels often differ pretty widely in direction from the adjacent striation, and the end nearest the northeast commences as a rounded depression without the clear cut edge presented by the striated scoop-marks. The darkly shaded spot in fig. 6 was deeper than the rest, as if from a species of "pot-hole" action.

6.



Most of the striated scoop-marks are not independent phenomena, like fig. 6, but evidently are appendages to adjacent knobs and projections of rock. The type of such cases is represented in fig. 7, which is a diagram of the plan of numerous specimens seen along the eastern shore from Killarney to Parry Sound, a distance of about one hundred miles. K is a boss or knob of rock projecting above the general glaciated surface in an oval form —in fact, a *roche moutonnée*. The specimen in fig. 7 is about four feet high, fifteen wide and thirty long; but all sizes and irregular forms are common. The observed specimens are mostly in gneiss. The horizontal lines in the diagram show the general direction of the striation in the locality — nearly west-southwest, as shown by the arrow. It will be seen that as the striæ approach and rise upon the surface of the knob they are deflected to the right and left and sweep over it in an oblique course. This sort of curved deflection, partly over and partly around obstacles is common to the whole coast, so that in many places almost all the striæ are curved by the influence of the knobby surfaces of the gneiss and quartzite.

7.



T T are two unstriated scoop-marks having a length of about ten feet and a width of twelve inches. They begin vaguely near to each other, but not in contact, close to the northeast end of the knob, and rapidly deepen to six inches or more as they curve about its two sides, after which they grow wider and more shallow until they become lost in the general glaciation along the sides of the rocky buttress. There are no striæ in these concavities and their inner borders do not touch the base of the knob but keep a foot or more away from it, leaving a narrow strip of level striated rock between the margins of the troughs and the eminence.

In a multitude of cases like this it is sufficiently clear that the resistance offered by knobs of rocks to the progress of the drift agencies in some way determined the presence and direction of these scoop-marks.

The great belt of fifty-two thousand islands, above referred to, varies from three to fifteen miles in width and is about two hundred and fifty miles long. Beginning at the St. Marie River it first outlines the north channel by the great Manitoulin group, and thence, passing southeast through Frazer Bay, continues along the whole east coast of Georgian Bay and terminates near Collingwood. Near the main land the islands are mostly metamorphic with a very distorted stratification, but a few of those on the lakeward border of the belt are of Silurian limestone with the strata dipping gently away from the nearest metamorphic hills.

This almost untrodden solitude, which has lain forgotten by the crowds of summer pleasure seekers, is well worthy of a visit by the lover of nature. The magnificent panoramas of the island-belt as viewed from the summits of the La Cloche and Killarney ranges are unique and in themselves well worth the journey to the region. Fortunately they are as yet almost unknown to sight-seers and still remain in their original freshness and silence.

Art. XIII.—*Response to the Remarks of Messrs. Wachsmuth and Springer on the genera Glyptocrinus and Reteocrinus;* by S. A. Miller.

In response to the remarks of the distinguished paleontologists, Messrs. Wachsmuth and Springer, on the genera *Glyptocrinus* and *Reteocrinus*, in the April No. of this Journal, p. 255, I would say, that the first issue joined, between us, is one of law and not of fact and may be stated as follows:

In 1858, Can. Org. Rem., Decade 4, p. 63, Prof. Billings defined the genus *Reteocrinus* and the species *Reteocrinus stellaris* from the Trenton Group, at Ottawa, Canada, and also, with much doubt referred another species to the same genus. In 1866, in advance sheets of the 24th Rep. N. Y. St. Mus. Nat. Hist. p. 206, Prof. Hall defined the species *Glyptocrinus Nealli*, from the upper part of the Hudson River Group, at Lebanon, Ohio. In 1881, Revision of the Palæocrinoidea, pt. 2, p. 191, Messrs. Wachsmuth and Springer reconstructed the genus *Reteocrinus*, with *Glyptocrinus Nealli* as the type, and, in their remarks above referred to (p. 264), they defend their right to retain a generic name and substitute any species as the type of the genus (always of course having a good specimen from which to ascertain the characters). If, they say, "It happens, that the true characters of the group are better and more comprehensively expressed in some other species than the one first described, there is, in our opinion, not the least objection to adopting it as the type of the genus thus rectified." They think their practice, in this respect, fully justified, and aver they intend to adhere to it, and say they find "other good authorities do the same thing."

The question is, Can a subsequent author revise a genus and substitute as its type a species unknown to the original author of the generic name, or not included by him, among his types of the genus, or not, itself, originally made or intended to be made the type?

I deny that the subsequent author has any such privilege, under the laws and rules of science, and affirm that no matter how learned the naturalist, how eminent the scientist, or inherently able and accurate the diagnosis may be, such work is an absolute nullity.

The issue is then, as before remarked, one of law, and the importance of determining it is not confined, in its scope, to the genera under consideration, nor to paleontology, but extends throughout Natural History. If I am right and should be able to convince Messrs. Wachsmuth and Springer, that such is the case, there is no doubt, they would, notwithstanding previous opinions, follow the law, because they are not only learned paleontologists but devotees of science.

I will quote from the rules for rendering the nomenclature of Zoology uniform and permanent adopted at the 12th meeting of the British Association for the Advancement of Science in 1842.

"It being admitted on all hands that words are only the conventional signs of ideas, it is evident that language can only attain its end effectually by being permanently established and generally recognized. This consideration ought, it would seem, to have checked those who are continually attempting to subvert the established language of Zoology by substituting terms of their own coinage. But, forgetting the true value of language, they persist in confounding the *name* of a species or group with its *definition;* and because the former always falls short of the fullness of expresion found in the latter, they cancel it without hesitation, and introduce some new term which appears to them more characteristic, but which is utterly unknown to the science, and is, therefore, devoid of all authority. If those persons were to object to such names of men as *Long*, *Little*, *Armstrong Golightly*, etc., in cases where they fail to apply to the individuals who bear them, or should complain of the names *Gough*, *Lawrence*, or *Harvey*, that they were devoid of meaning, and should hence propose to change them for more characteristic appellations, they would not act more unphilosophically or inconsiderately than they do in the case before us; for, in truth, it matters not, in the least, by what conventional sound we agree to designate an individual object, provided the sign to be employed be stamped with such an authority as will suffice to make it pass current. Now, in Zoology, no one person can subsequently claim an authority equal to that possessed by the person who is the first to define a new genus or describe a new species; and hence it is, that the name originally given, even though it may be inferior in point of elegance or expressiveness to those subsequently proposed, ought as a general principle to be permanently retained. To this consideration we ought to add, the injustice of erasing the name originally selected by the person to whose labors we owe our first knowledge of the object; and we should reflect how much the permission of such a practice opens a door to obscure pretenders for dragging themselves into notice at the expense of original observers."

"The name originally given by the founder of a group, or the describer of a species, should be permanently retained to the exclusion of all subsequent synonyms."

"As the number of known species which form the groundwork of zoological science is always increasing, and our knowledge of their structure becomes more complete, fresh generalizations continually occur to the naturalist, and the number of genera and other groups requiring appellations is ever becoming more extensive. It thus becomes necessary to subdivide the contents of old groups, and to make their definitions continually more restricted. In carrying out this process, it is an act of justice to the original author that his generic name should never be lost sight of; and it is no less essential to the welfare of the science, that all which is sound in its nomenclature should remain unaltered amid the additions which are continually being made to it."

"A generic name, when once established, should never be cancelled in any subsequent subdivision of the group, but retained in a restricted sense for one of the constituent portions."

"When a genus is subdivided into other genera, the original name should be retained for that portion of it which exhibits in the greatest degree its essential characters as at first defined. Authors frequently indicate this by selecting some one species as a fixed point of reference, which they term, the 'type of the genus.' When they omit doing so, it may still in many cases be correctly inferred that the *first* species mentioned on their list, if found accurately to agree with their definition, was regarded by them as the type. A specific name or its synonyms will also often serve to point out the particular species, which by implication must be regarded as the original type of a genus. In such cases we are justified in restoring the name of the old genus to its typical signification, even when later authors have done otherwise."

"The generic name should always be retained for that portion of the original genus which was considered typical by the author."

"*Example.*—The genus *Picumnus* was established by Temminck, and included two groups, one with four toes, the other with three, the *former* of which was regarded by the author as typical. Swainson, however, in raising these groups at a later period to the rank of genera, gave a new name, *Asthenurus*, to the *former* group, and retained *Picumnus* for the *latter*. In this case we have no choice but to restore the name *Picumnus* Tem., to its correct sense, canceling the name *Asthenurus* Sw., and imposing a new name on the three-toed group which Swa had called *Picumnus.*"

"When no type is indicated, then the original name is kept for that subsequent subdivision which first received it."

"When the evidence as to the original type of a genus i perfectly clear and indisputable, then the person who first s vides the genus may affix the original name to any portion at his discretion, and no later author has a right to transfei name to any part of the original genus."

"When an author infringes the law of priority, by givi new name to a genus which has been properly defined and n already, the only penalty which can be attached to this a negligence or injustice is to expel the name so introduced the pale of science."

"When two authors define and name the same genus, making it exactly of the same extent, the later name shou canceled *in toto*, and not retained in a modified sense."

"No special rule is required for the cases in which the lat two generic names is so defined as to be *less extensive* in sign tion than the earlier, for if the later includes the type of the e genus, it would be canceled by the operation of the rule tha generic name should always be retained for that portion o original genus which was considered typical by the author."

"If the later name be so defined as to be equal in exte two or more previously published genera, it must be canc *in toto.*"

"A genus compounded of two or more previously prop genera, whose characters are now deemed insufficient, sh retain the name of one of them. If these original generic n differ in date, the oldest one should be the one adopted."

The committee on zoological nomenclature, consistin Wm. H. Dall, who was assisted by Dr. Asa Gray, appoi by the American Association for the Advancement of Sci in 1876, reported to the Nashville meeting in 1877. Upon subject of names to be preserved in writing, dividing or m fying the limits of existing groups, the committee said:

"§ L. A change in the diagnostic characters, or a revi which carries with it the exclusion of certain elements of a gr or the inclusion of new elements, does not authorize the chang the name or names of a group."

"§ LI. When a group or genus is divided into two or n groups, the original name must be preserved and given to on the principal divisions. The division including the typical spe of the primitive genus, if any type had been specified, or ldest, best known or most characteristic of the species origin cluded when the primitive genus was first described by uthor, is the portion for which the original name is to be erved. If there is no section specially so distinguished, t which retains the larger number of species should retain the old name (DC.), but the latter cannot be applied to a restricted group containing none of the species referred to the primitive group by its author at the time when it was described or when he enumerated the species contained in it."

In the preparation of this report the author placed great dependence on the memoir of DeCandolle, and presented to the working naturalists a series of questions, requesting answers, which were quite fully responded to. The 12th question and the answers were as follows:

"Shall a subsequent author be permitted in revising a composite genus (of which no type was specified when it was described) to name as its type a species not included by the original author of the genus in that latter author's list of species given when the genus was originally described? No 37. Doubtful 2. Yes 5. No answer 1."

I will cite only one more authority. Professor Meek in discussing the type of the genus Straparollus says:

"It seems to us, however, that if the name *Euomphalus* is to be retained at all, we should apply it to the forms for which it was originally proposed, and that we have no right to transfer it to another type, because Sowerby subsequently, in another place, refers this other type to his genus *Euomphalus*."—Geol. Surv. Ill., vol. ii, p. 158.

The rule that a subsequent author cannot revise a genus and substitute as its type a species different from that relied upon by the founder of the genus seems to be as well settled in England and America as any other rule or law upon the subject of nomenclature in Natural History, and I know of no author of recognized merit in either country, save Messrs. Wachsmuth and Springer, who has attempted to violate it. The subject is not new, and the instances of strictly adhering to the rule, under circumstances where it would have seemed to accommodate the author to violate it, are numerous. For instance, Professor Hall, mistaking the type of the genus *Retzia*, proposed and defined the genus *Rhynchospira;* afterward ascertaining that *Rhynchospira* was a synonym for *Retzia*, he abandoned it and proposed *Rhynchotreta* for the form which he had originally mistaken for *Retzia*. Had it not been for this rule he might have abandoned *Retzia evax* as the type of his genus *Rhynchospira*, and substituted *Rhynchonella cuneata*, which became the type of *Rhynchotreta*. You will look in vain in the works of such authors as Hall, Billings, Cope and Meek for any support of the views of Wachsmuth and Springer upon this subject; and the reason is obvious. If they can substitute another than the original species as the type of a genus, I can substitute yet another, and you can another, and so we destroy all *fixity* in the type and designated characters, throw the science into confusion, and seriously impair the value and reliability of generic characters, besides losing all interest in the original work of the author who established the genus.

The conclusion is inevitable that, as a matter of law or rule of science, *Glyptocrinus Nealli* cannot be made the type of the genus *Reteocrinus*, and the diagnosis must be canceled and stricken from the pale of science.

Nor are we in accord in respect to the value of the genus *Reteocrinus*, nor as to the relationship existing between it and *Glyptocrinus Nealli*, for I regard the latter as having a closer affinity to *Glyptocrinus decadactylus* than it has to *Reteocrinus stellaris*, though it may fairly be regarded as holding an intermediate position. I have had the pleasure of seeing the original specimen represented by fig. 4*a*, pl. 9, of Decade 4, which is the type of the genus *Reteocrinus*, and from recollection think the figure is a correct representation of it. Now let us compare it with *G. Nealli;* of course the comparison can only be made with the azygous side and the column.

G. Nealli.—Column sharply pentagonal and composed of alternating thin and thicker pieces.

R. stellaris.—Column round and composed of very thin plates.

G. Nealli.—Basal plates very small, presenting a low triangular face on the exterior (though minutely truncated at the lateral angles) and not interfering with the pentalobate character, as viewed from below, in following the depressions in the column across the central part and greatest height of the basals and beyond the lower lateral sides of the subradials.

R. stellaris.—Basals large, presenting an hexagonal face on the exterior and bearing a strong bow-shaped ridge with sinus or concave side upward, not in contact with the margins, except where it meets corresponding ridges on the succeeding plates above; the remaining portions of these plates depressed from ½ to ¾ of a line. No pentalobate character in the central part of these plates, but deep depressions at their lower lateral sides.

G. Nealli.—Subradials about as wide as long, except the one on the azygous side which is a little longer than wide, and each bearing a semicylindrical three-rayed ridge, highest in the central part and sending one arm below to meet the angles of the column and one to each of the adjoining radials to meet corresponding semicylindrical ridges, except the subradial, on the azygous side, which bears an additional depressed semicylindrical ridge extending upward to the superior truncated side.

R. stellaris.—Subradials very large, longer than wide, and each bearing a more than semicylindrical double-bifurcated or four-rayed ridge, two of the ridges on each uniting with the concave ridges on the basals below and two uniting with similar ridges on the primary radials above. The subradial on the azygous side bears a fifth or additional ridge extending upward from the middle of the concave side of the upper bifurcating ridges to the superior truncated side or middle of the azygous interradius. The depressions between these plates or the height of these ridges is ¾ of a line.

G. Nealli.—Primary radials three in each series, except in the left posterior ray, which has only two. These plates are slightly longer than wide, the first and third, or in the left posterior ray the first and second, are pentagonal and of almost exactly the same size, form and ornamentation, so that by reversing ends they would almost fill each other's places. The second plate in four series is subquadrangular.

R. stellaris.—Primary radials four in each series, the first one very large, bearing a high bifurcated ridge which unites with corresponding ridges on the subradials on either side below; the second much smaller and shorter; the third diminished in size a little more; and the fourth very small in comparison with the first, evidently not more than one-fourth as large.

G. Nealli.—Secondary radials, twelve to about sixteen in each series, four or five of the lower ones being larger and proportionally longer, than those above (which are very short, free, and scarcely differ from the free arm pieces); the second or third one having somewhat the character of an axillary piece, and giving off a small lateral division or series of pieces on the outer or interradial side, that does not become free, but is soldered in with the interradials to the top of the body where it gives origin to a pinnule like those of the arms.

R. stellaris.—Secondary radials, four to six in each series very gradually diminishing in size, without any evidence of a lateral division from either plate.

G. Nealli.—Azygous interradial area covered by fifty or sixty plates, very unequal in size, the middle row being decidedly larger and more prominent than the others, so as to form a ridge up the middle, while the other smaller and less prominent ones are crowded in, irregularly, on each side. The plates in this middle row, however, have no uniformity in size or shape; the first one is large and elongated, the fourth is small and subquadrate; and the row has become about obsolete at the sixth plate, where all are of nearly the same size and scarcely distinguishable from the minute pieces which cover the flattened vault and with which they unite.

R. stellaris.—Azygous interradial area covered by a large number of plates, probably one hundred or more, very unequal in size, the middle row being decidedly larger and more prominent than the others so as to form a ridge up the middle. The plates in this row, however, do not rapidly diminish in si and fade out in their distinctive character before reaching th top of the vault; on the contrary they are longer than the pr mary radials, four of them reach nearly as high as the last o the secondary radials, and while the specimen is not preserve above this, enough is disclosed to the paleontologist to show that this series continued up the face of a proboscis that ex tended may be as far or farther than the arms and the pinnules

If we look at the general aspect and form of *G. Nealli* an *R. stellaris* there is as little resemblance as we have found in th more particular comparison. *G. Nealli* has a pentalobate obc noidal calyx and large flowing arms and pinnules. *R. stellar* has a saucer-shaped calyx below, it is elongated in the regio of the primary radials and has small or diminutive arms an pinnules. Indeed there is no striking resemblance between th two species, except in the depressed interradial areas covere by numerous plates, but even this resemblance is not continue in the upward extension of the azygous interradial area.

Therefore, notwithstanding the learning and usually skillfu judgment of Messrs. Wachsmuth and Springer in regard t fossils belonging to this class, fortified as it is by the opinion o my young friend Walter R. Billings and by Professor A. G Wetherby, and the confidence with which these authors asser that "The question of the generic identity of *G. Nealli* an allied forms with *Reteocrinus*, may be considered at rest," I believe, and express the opinion without hesitation, that they ar *not* congeneric, and that *Reteocrinus stellaris* is so far remove from *Glyptocrinus Nealli* that it is doubtful whether they shoul even be classified in the same family.

Messrs. Wachsmuth and Springer call attention to the fa that in describing *Glyptocrinus Pattersoni* I said it differe from other species of *Glyptocrinus* in having only ten arms there was a thoughtless omission on my part to except *Glypt crinus Baeri*; but they say *Glyptocrinus Nealli* and *Glyptocrinu cognatus*, which I had described only a year before, have on ten arms. This is to me incomprehensible, for *Glyptocrinu Nealli* was described by Hall and again by Meek (Ohio Pa vol. i, p. 35) as a species bearing twenty arms, and I have se the type, and have examined more than a hundred specimen each having twenty arms and never saw a specimen witho that number. As to *Glyptocrinus cognatus*, the number of arm was unknown, at the time the species was described, but I su pose it to have had either twenty or twenty-four. I have no before me three specimens so nearly allied to it that it is ha to point out specific differences. Each of them show the arm and each have twenty-four.

I do not desire to be understood as saying that it is eith inexpedient or improper to separate the species now arranged under *Glyptocrinus* and distribute them in two or three genera; no satisfactory work, however, has yet been done in that direction; and if *Glyptocrinus Nealli*, *G. Baeri*, *G. subglobosus* and *G. fimbriatus* are thrown in one section, then I am free to say it is better that all shall remain under the present generic name. Moreover, I think subgeneric names in paleontology ought not to be encouraged, because they are inconsistent with the binomial system of nomenclature and really serve no scientific purpose.

To review the species now classed with *Glyptocrinus* and point out their differences in structure would too greatly lengthen this article. It may be done in another communication.

ART. XIV.—*On the Present Status of the Eccentricity Theory of Glacial Climate;* by W. J. McGee.

THE recent appearance of an important treatise, in which Croll's theory of secular variations in terrestrial climate is given a prominent place,[1] has elicited some adverse criticism of that theory, by different reviewers, which can only be regarded as embodying the current objections to the adoption of the eccentricity theory in general.

In addition to an indefinite general argument such as might equally be brought to bear against any intricate and comprehensive theory involving principles falling within the domains of diverse nascent branches of science, Gilbert[2] urges three definite and specific objections against the theory:

1. "If it is true, then epochs of cold must have occurred with considerable frequency through the entire period represented by the stratified rocks; and iceberg drift, if no other traces, should have been entombed at numerous horizons. It has not been found, however, and of the eight horizons claimed by Croll to show evidence of glacial action, the treatise under consideration mentions only two with confidence, and two others with doubt. In the two instances to which queries are not attached, the phenomena appear to indicate local and not general glaciation.

2. If the hypothesis is true, the cold of the Glacial epoch must have been many times interrupted by intervals of exceptional warmth; but little has been added to the evidence adduced by Croll for such an interruption, and in America, where there is now great activity in the investigation of glacial phenomena,

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[1] "Text Book of Geology," A. Geikie, 1882, 21–29. [2] Nature, xxvii, 262.

the evidence of a *single* inter-glacial period is cumulative and overwhelming, while there is no indication whatever of more than one.

3. If the hypothesis is true, submergence in polar and temperate regions should have been coincident with glacial expansion, and emergence coincident with glacial retreat, but the Quaternary history of Great Britain, as drawn in the new text-book, includes two periods of maximum ice-extension, *separated* by a period of submergence."

The editor of the American Naturalist[1] insists 1, that the "hypothetical stoppage of the Gulf Stream to account for the glacial climate of Northern Europe is not warranted by paleontological facts," and 2, that—"So extremely hypothetical, from paleontological considerations, is the evidence of the so-called 'interglacial periods,' * * that we wonder that our author should endorse Dr. Croll's speculations without stating some of the facts supposed to sustain such a view."

With the conservatism characteristic of British specialists, Lapworth,[illegible] in a seven-word clause, relegates the theory to the domain of "attractive speculation."

N[illegible] from the most eminent [illegible] indisputable facts, [illegible] determining the [illegible] against which they are directed; [illegible] eccentricity theory except in so far as [illegible] and is now advocated by Dr. Croll [illegible] regarded by general students as [illegible]. It is therefore most [illegible] unquestioned, that the [illegible] should be unmistak[illegible] reviews should present [illegible] the various phases [illegible] they are apparently de[illegible] these [illegible] do not appear [illegible] theory, as now em[illegible] framed to [illegible] appears to him [illegible] be impressed [illegible]

S[illegible] of the cold of the [illegible] precession, and [illegible] glacial conditions [illegible] has been admirably [illegible] yet received [illegible] It is there

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[illegible]

[illegible] Vol. VIII, [illegible]

established that, as long ago suggested by Lyell, continental configuration must exercise an important influence on the accumulation of land ice, and that the pre-Quaternary geography of the northern hemisphere was probably such as to preclude extensive ice-accumulation. In this view the recognized fundamental elements of glacial climate are eccentricity, precession and continental configuration (to which should be added, secular refrigeration). Such view has already been favorably received by LeConte,[7] Penck[8] and others, and no reason to question its truth has thus far appeared.

The summary objection first enumerated is therefore invalid.

The common second objection of the first two reviewers has also been anticipated by Wallace, whose climate-diagram exhibits but one a-glacial period[9] separating two considerable glacial periods.[10] The improbability that an extensive ice-sheet could be melted during an inter-glacial epoch had been previously shown, quantitatively, by the writer.[11] Since, however, these investigations have been alike neglected in the recent reviews, a more thorough examination must be undertaken.

While the maximum accumulation of polar ice must ever fall below that of middle latitudes,[12] and while independent glaciers may be locally developed in any latitude, provided other conditions be favorable, it may be assumed without discussion that the Quaternary ice of the northern hemisphere originated well within the arctic circle; and here, accordingly, may the manner and rate of development of an ice-sheet be considered.

Again: while glacial periods must, as is generally admitted, be periods of low temperature, and must also, as long ago demonstrated by Tyndall,[13] be characterized by abundant precipitation, the recognition of these conditions as causative and not induced is unwarranted in the present state of knowledge; while, as best elucidated by Wallace,[14] geographical vicissitudes unquestionably affect the formation of polar ice, the evidence of such changes of magnitude commensurate with the phenomena of Quaternary glaciers wholly fails; and while as urged by Croll,[15] continental configuration may serve to indirectly intensify glacial conditions, its agency must ever be secondary and subordinate. In the eccentricity theory, *per se*, then, all sav the immediate effects of increased eccentricity, under conditions similar to those actually known to obtain, must be eliminated; and on these further premises may the mode and rate o ice-accumulation be sought.

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[7] "Elements of Geology," 2d ed., 1882, 578.

[8] "Die Vergletscherung der Deutschen Alpen." 1882, 452.

[9] Since the term "inter-glacial" was used in a definite and restricted sense by Croll, it seems desirable that some other expression should be employed to denote any considerable period, or number of periods, during which glaciers did not prevail; and for this purpose "a-glacial" is suitable.

[10] "Island Life," 122.

[11] Geol. Mag., vi, 1879, 418.

[12] Cf. "Maximum Synchronous Glaciation," Proc. Am. Assoc. Adv. Sci., xxix, 1880, 447, *et seq.*

[13] Cf. "Forms of Water," 1877, 154.

[14] "Island Life," Ch. IX.

[15] "Climate and Time," Am. ed., 1875, Ch. v; Geol. Mag., vi, 1879, 480; Geikie's Text Book, 1882, 27; and elsewhere.

In the north-frigid zone the existing ice-fields are to all appearances permanent; whence annual addition to them fron congealed precipitation and loss from melting, flow of ice an water, and the liberation of bergs are practically equal. Th annual precipitation can only be approximately estimated. I on the last edition of Loomis' rain chart[16] the precipitation o land areas be the means of the values represented by the seven tints employed, the average for the year at N. lat. 68° is 13· inches. Toward the pole it must be materially less: it is, indeed, sometimes so little in northern Greenland that Bessel thought the glaciers there must be but remnants of those forme during past ages.[17] The mean (and the measure of meltin for the whole year certainly cannot exceed 10 inches.

Neglecting trivial amounts from diverse sources, the hea reaching the frigid zone is derived (1) from vapor-laden wind and (2) from direct solar accession. Now that received fro the first of these sources is indeterminate; but that from th second is alone sufficient to liquefy 399 inches (33·26 feet) annually. Actually not more (and probably far less) tha $\frac{1}{40}$ of this melting can take place, and it is hence manifest tha in computing the effects of eccentricity, the actual and not th theoretical values of annual addition and loss must be employed.[19] The source of the discrepance need not here be considered in detail.

For convenience, and since no appreciable error will be introduced thereby, the foregoing values for precipitation and melting, and their equality, may be assumed normal—i. e., such a would obtain were the solstices equidistant from the apsides.

Different investigators have shown that the immediate resul of increased eccentricity (in conjunction with precession) mus be the inauguration of five glaciation-factors, of which two are direct and three indirect. These are, (1) diminution of mean temperature,[20] (2) increase of evaporation and precipitation,[21] (3) acceleration of radiation,[22] (4) promotion of reflection,[23] and (5) increase of protection from solar accession by clouds and fogs.[24] The first factor has been roughly evaluated only as regards its effect on temperature, whence its agency in ice-accumulation remains indefinite; and the quantitative efficiency of the last four has never been sought, and cannot now be numerically expressed without more thorough investigation than is here warranted. It must therefore suffice to assume for all a probable value;[25] and it will assuredly do no violence to the most conservative opinions (especially in view of the considerable influence shown to be exerted by the first factor) to assume that the united agency of the five factors is such as to counterbalance the lessened solar distance in summer, and render the combined factors effective in the sum of the excess of winter-season above normal. Manifestly, since but two (and probably not the most important) of the glaciation-factors are directly effective, while the others acquire efficiency only through the accumulation of ice, the diminution of annual melting will not be uniform but cumulative throughout the Platonic winter—the period during which winter-season occurs in aphelion; as the ice increases, so will its disposition to increase become intensified; but the assumed may be regarded as the mean rate. During each Platonic winter, then, a mean accumulation of ice directly proportional to the increase in length of winter-season will annually take place.

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[16] This Journal, xxv, January, 1883.

[17] Cited by Woeikof. "Winds of the Globe," Smithsonian Contrib. Knowl., 26 1875 (=vol. xx, 1876). 680.

[18] Cf. "Maximum Synchronous Glaciation," op. cit., 473.

If, as appears from a popular notice (Harper's Magazine, lxvii, 1883, 91, c this Journal, xxv, 1883, 195), Langley's recent observations prove (1) that th value of solar accession hitherto accepted is too low, and (2) that the temperatur of stellar space approaches absolute zero, the argument here presented will gai in force, and greater potency must be ascribed to the agencies contemplated in th eccentricity theory.

[19] In the discussion already alluded to ("On the Superficial Deposits of the Mi sissippi Valley," Geol. Mag., vi, 1879, 418) the theoretical rate of melting wa used as a basis for an estimate of the maximum removal of ice during an interglacial period; and the result is accordingly far too large.

With the eccentricity of 210,000 years ago (·0575, as computed by Croll)[26] the maximum excess of winter-season over the normal during such Platonic winter was about 13·5 days, or a mean for each such period of 6·75 days; throughout which period, accordingly, the annual accumulation of ice was $\frac{6{\cdot}75}{365} \times 10$, or ·185 inch, and within which the total accumulation was equal to ·185×13,000, or 2405 inches (200 feet).

Toward the equator actual and possible annual precipitation and liquefaction progressively increase, the first four glaciation-factors progressively lose in relative efficiency, the fifth increase the share of heat derived from warmer latitudes diminishes, an the periodicity of solar accession becomes more equable, whereb loss through radiation is accelerated. From this complex o diverse and antagonistic elements only the most vague estimate of the relative rates of addition and loss in higher and lowe latitudes could be directly deduced without exhaustive analysi and computation; but it is certain that the annual addition t the ice-sheet could never exceed the precipitation, while it i obvious that the annual loss must fail of the addition; whenc the foregoing value, if doubled or tripled, and certainly if quadrupled, would be ample for the whole glaciated area of th northern hemisphere.

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[20] The writer, "A Contribution to Croll's Theory," this Journal, xxii, 1881, 437.

[21] Hill, "Evaporation and Eccentricity," Geol. Mag., viii, 1881, 481; this Journal, xxiii, 1882, 61.

[22] Croll, "Climate and Time," 1875, 58; Geikie's Text Book," 1882, 25.

[23] Croll, op. cit. 1, 60; op. cit. 2, 26.

[24] Croll, op. cit. 1, 60; op. cit. 2, 26.

[25] Let it be clearly understood that such an assumption is not made as an attempt to demonstrate the validity of the eccentricity theory by any process of defective reasoning; the only demonstration worthy of the name, now admissible, would be an approximate evaluation of the glaciation-factors, severally and jointly, at the various latitude of the zone over which they are efficient. Such an investigation presents no serious difficulty aside from the time and labor required in its prosecution.

[26] "Climate and Time," 320.

But however slow the rate of ice-accumulation, the rate of ic dissolution, when the positions of the hemispheres were reverse would be far slower; for the effective factors of accelerate radiation and promoted reflection, which are but secondary i the initial development of an ice-sheet, would then reach maximum efficiency. The enormous dissipation of heat by icy surfaces is seldom adequately appreciated: after a light snow-fal equal to but a fraction of an inch of ice, in the upper Mississipp valley, the temperature falls from freezing-point to zero, and th snow is not even softened by a day's uninterrupted sunshin demonstrably sufficient to melt an inch and three-quarters o ice; the névé-fields of the Savoyan Alps receive enough sola energy in a year to melt 54 feet of ice, yet the actual superficia liquefaction must be trivial; an earlier paragraph indicates tha less than a fortieth of the theoretical melting actually occurs i the frigid zones; the solar accession in the frigid zone in summer is considerably greater than at the equator, as Meech ha calculated,[27] yet the liquefaction annually effected there woul be effected in a week were the available energy utilized in suc work; it appears susceptible of mathematic proof that if th water of the earth were converted into a mantle of ice uniforml enveloping its surface, not a crystal of it would ever be melte Not even an approximate estimate of the rate of ice-dissolutio can be made without analyzing and evaluating the wonderfull effective agencies of radiation and reflection; but it is certai again that only a fraction of the ice accumulated during Platonic winter could be removed during a Platonic summer and whether the fraction be large or small, it may safely b affirmed that little more than a tithe of the Quaternary ice-she could have been removed during a single inter-glacial period.

Let it be observed that if the preceding estimates of prese annual precipitation, or of the combined values of the glaciation-factors, be excessive, the computed rate of ice-accumulation is too rapid; while if they be defective, the importance and efficiency of eccentricity as an element in glacial climate has been underestimated.

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[27] "Relative Intensity of the Light and Heat of the Sun," Smiths. Contr Knowl., 85, 1855 (=vol. ix, 1856), 18, pl. I.

The foregoing results sustain the opinion of Wallace and others as already stated, and show that (presumably) the weightiest objection of the recent reviewers is invalid.

The third objection of the first reviewer is based on a subordinate side-question springing indirectly from the Crollian theory, which may or may not in any way affect the fundamental principles of the theory; for the question as to what physical effect a given mass of ice will exert on the earth's center of gravity and on the position of the ocean is wholly independent of the question as to the reason of Quaternary ice-accumulation; and the validity of the eccentricity theory, *per se*, is accordingly in no way affected by the verity of the phenomena adduced.

Again, the tripartite sequence of Quaternary deposits described in the text-book (glacial—aqueous—glacial) does not appear to be so thoroughly understood and so clearly drawn, and the consensus of opinion concerning it so uniform, as to allay the suspicion that the aqueous beds may be analogous to those everywhere deposited during and immediately after the withdrawal of the second ice-sheet.

This objection, therefore, is also invalid.

In his first objection the second reviewer overlooks the fact (upon which the writer has already had occasion to insist)[26] that the hypothetical deflection of the Gulf Stream, in the manner contemplated by Croll, is an *effect* of glaciation, and, if a cause at all, only a secondary one. Hence if the glaciation-factors alone are capable of inaugurating a glacial period, the assistance of this element is not essential; and if they are not alone capable of producing such an effect, the whole theory fails.

This objection, too is accordingly incompetent.

With no desire to underestimate the actual difficulties of the eccentricity theory, or to detract one iota from the laudable caution displayed in such general criticism as that of the first reviewer, a word may be added with reference to the depreciatory tone of the class of critics represented by the third reviewer. Intricate and far-reaching as the theory is, encroaching as it does upon different branches of science, involving as it must elements seldom coördinated by individual specialists, it requires for its thorough comprehension a range of preliminary study which few geologists can afford to bestow upon it; and as with special investigations generally, so in this case, the men who have not made such study are prone to ignore or disparage both the investigation and its results. It assuredly speaks strongly for the respectability, and equally makes for the probability of the theory, that nearly every geologist whose writings show that he thoroughly comprehends it is disposed to regard it as something more than a vague hypothesis, and that those who understand its principles best are most ready to teach it as a tentative but probable geologic and cosmogonic doctrine. Never more, and seldom as much, may be said of the narrower speculations of empirical geology.

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[26] "Croll's 'Climate and Time'" (review), Popular Science Monthly, xvi, 1880, 819.

Salt Lake City, Utah, April 15th, 1883.

ART. XV.—*On the Commingling of ancient Faunal and modern Floral Types in the Laramie Group*; by CHARLES A. WHITE.

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THE fact that fossil plants of Tertiary types have been found in strata of the Laramie Group associated with Dinosaurian remains has been long known and undisputed, even by those who strongly contend for the Cretaceous age of that group. Concerning the true Laramie age of some of the collections of plants, however, that have been published as coming from strata which are now known to belong to that great group, many of those who are interested in the matter have felt some doubt, and others have persistently discredited it. In the present consideration of this question I shall make special reference to those strata of northeastern Montana and adjacent parts of Dakota, to which Dr. Hayden gave the name of Fort Union Group, and to the large collection of plants which he made from its strata. Descriptions of these plants were published by Dr. J. S. Newberry, in 1868, in volume ix of the Annals of th New York Lyceum of Natural History, pp. 1–76; and hi illustrations of the same were published by Dr. Hayden, in 1878, in connection with the U. S. Geological Survey of the Territories. Dr. Newberry then referred the species of the plants in question to the Miocene epoch; and while most of them were then new, he identified a part of them with know European Miocene species. So distinctly characteristic of the Tertiary period were the types of those plants generally admi ted to be that the assignment which Dr. Newberry made to them has, I believe, never been seriously called in question. Dr. Hayden at the same time and from the same localities, col lected many invertebrate fossils which were afterward described by Mr. Meek and himself; and some years afterward Professor Cope obtained from the same region numerous vertebrate remains, among which were those of Dinosaurs. Mr. Clarence King, accepting the conclusions of Dr. Newberry as to the Miocene Tertiary age of the plants here referred to, and those of Professor Cope as to the Cretaceous age of the Dinosaurs, expressed the opinion that the strata from which the former were obtained were distinct from those of the latter and of much later geological age. The following paragraph is quoted from his volume I, of the U. S. Geological Survey of the 40th Parallel, p. 353: "The relations of conformity or nonconformity between the plant-bearing beds of Fort Union, and the Dinosaurian beds are not given, and there is reason to believe that the plant beds represent a horizon of the great White River Miocene series which underlies the Pliocene over so large a part of the Great plains. * * * * I apprehend that the plant-horizon at Fort Union will be found to be nothing but the northward extension of the White River Miocene." My reason for referring especially to this statement of Mr. King is that it occupies a place in the leading volume of one of the larger series of U. S. Government geological publications, and it is therefore (although he distinctly states that he has never visited the region in question), liable to mislead those who have not personally studied the geology of that region, if it should pass unchallenged.

During the summer of 1882 I gave especial study to the geology of the region about Fort Union, extending up the Yellowstone Valley, and including all the localities from which Dr. Hayden obtained the fossil plants here referred to. The result of that study has been to ascertain that only one formation, namely, the characteristic Fort Union Group, which is nothing more or less than a part of the great Laramie Group, occupies that whole region. That is, with the exception of one or two small exposures of the Fox Hills Cretaceous Group, upon which the Laramie strata rest conformably, no other than Laramie strata are to be found there.*

At numerous points along the valleys of the Yellowstone and Missouri, including many of Dr. Hayden's localities, I collected fossil plants, shells and vertebrate remains. The molluscan shells are those which belong to characteristic species of the Laramie Group, examples of most of which I have collected from its strata in Wyoming and Colorado, on both sides of the Rocky Mountains. The following is a list of them as identified by myself, comparing them with the ty specimens or other authentic examples:

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* In the last May number of this Journal I gave an account of a small deposit which I found on the top of a butte about 100 miles south of Fort Union, which may be of later age than the Laramie Group; but in that deposit only a few fossil fishes were found, and it is several hundred feet above the fossil plant horizon.

Unio priscus Meek & Hayden.	*Viviparus Reynoldsianus* M. & H.
Unio senectus White.	*Viviparus Leidyi* M. & H.
Corbula mactriformis M. & H.	*Viviparus retusus* M. & H.
Thaumastus limnæformis M. & H.	*Viviparus Lleai* M. & H.
Viviparus trochiformis M. & H.	

Professor E. D. Cope, who has examined the vertebra remains which I collected with the foregoing mollusca, recognizes the former as belonging mostly to the same species which he had previously obtained from that region; and he gives me the following general determinations of them:

Dinosaurians, two species.
Campsosaurus, one species.
Chelonians, three species. (*Trionyx*, *Emys* and *Clastomenus*.)
Ganoids, one species. (*Clastes*.)

Mr. Lester F. Ward, who has charge of the fossil plants of the U. S. Geological Survey, and who is making a speci study of those of the Laramie Group, has given me a list of species which he has identified to his entire satisfaction wit species heretofore published. The larger part of these are species which Dr. Newberry published, as before mentione From that list I select the fourteen species named below for th purpose of illustrating the subject of this article:

PARTIAL LIST OF FOSSIL PLANTS OBTAINED FROM THE LARAMIE STRATA OF THE YELLOWSTONE VALLEY.*

Yellowstone valley.	*European.*	*Other Laramie localities.*
Equisetum arcticum Heer.	Miocene, Spitzbergen.	
Sequoia Langsdorffii Heer.	“ “	Black Buttes, Sout'rn Wy.
Phragmites Œningensis A. Br.	“ Switzerland.	“ “ “
Gymnogramma Haydenii Lesqx.	Golden Colorado.	
Sparganium stygium Heer.	“ Switzerl'd & Green'ld.	
Corylus Americana Walt. (living).		
Corylus rostrata Ait. (living).		
Platanus Guillelmæ Göpp.	“ Greenland.	
Magnolia tenuinervis Lesq'x.		Bl'k Buttes, Wy. and Go [den, Colorado
Elæodendron Helveticum Heer.	“ Europe.	
Rhamnus Gaudini Heer.	“ “	
Juglans rugosa Lesq'x.		Black Buttes, S. Wyom'g.
Juglans bilinica Ung.	“ “	
Trapa microphylla Lesq'x.		Point of Rocks, S. Wy.

Three important facts are shown by this partial list of th fossil plants from the Laramie strata of the Yellowstone valle

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* The other species of plants found associated with these are, as before state of recognized Miocene types, but they are omitted from this list because, so far the plants are concerned, reference is made especially to *specific* classification.

First, two of the species are among the most common living North American shrubs. Second, eight of them are identified with European Miocene species. Third, six of them are identified with those which have been found in unquestionable Laramie strata in Colorado and Southern Wyoming. Now, I assert without hesitation, that all the plants from which the foregoing list has been made were found in such association with the molluscan and vertebrate remains that have been mentioned in this article as to show plainly that they all lived contemporaneously. That is, if all these identifications are correct, as they are believed to be, two species of living plants, eight species of acknowledged Miocene plants, numerous fresh-water mollusks of living types, and two existing genera of Chelonians, all co-existed with Dinosaurs.

It is not to be denied, if there were any inducement to do so, that the Dinosaurian remains are found toward the base of the Laramie Group, in the Yellowstone valley, and that the principal horizon of the fossil plants is somewhat above that of the Dinosaurs there. But all the plants of the foregoing list have been found associated together, and some of them also extend below the upper known limit there of the Dinosaurs. Some of the other vertebrate remains herein named are found associated with the Dinosaurs, and the range of the former extends up into the principal horizon of the plants. There is no trace or suggestion of a separation of any of the strata which contain all these remains into separate formations, and there is no unconformity whatever among those strata. Furthermore, I have found at the Southern Wyoming localities named in the foregoing list of plants, Dinosaurian remains in layers above those which contain the plants. At least two of the latter, be it remembered, are identical with European Miocene species. The specific identity of the molluscan and plant remains leaves no possible room for doubt as to the equivalency of the Fort Union strata of the Yellowstone valley with the typical Laramie strata of Southern Wyoming that have been referred to.

ART. XVI.—*Notes on some Fossil Plants from Northern China;* by J. S. NEWBERRY.

MR. ARNOLD HAGUE recently placed in my hands a small collection of fossil plants brought by him from China. They proved to be interesting, and with his permission I present briefly the results of my examination of them.

The circumstances under which they were found, so far as known, are given in the subjoined notes of Mr. Hague which accompanied them;

"This collection of plants came from the coal basin of the Pin-hsu-hoo, in the southern peninsula of Mantchuria, on the east side of the Gulf of Liantung, and about one hundred miles northeast of the open port of Niu-chwang. I am told that at times there are over 1200 Chinamen engaged in the district in mining and transporting coal. This coal field has long been known to foreigners through the Chinese as a possible source of workable coal. As long ago as 1863 Professor Pumpelly suggested that the Liantung coals should be examined by American or European experts before opening the mines at Chaitang, which he had personally visited and reported upon favorably.

So far as I know no geologist has visited the district except Baron v. Richthofen, who regarded the formation as of Paleozoic age, although I believe he found no fossils.

From my own observations while traveling through the provinces of Chihte and Shansi, and from various sources of information, I believe by far the greater part of the coal basins of North China are of Paleozoic age, although the well-known districts west and northwest of Pekin have been shown to be of Mesozoic age.

The estimates of the great area and value of the coal and iron deposits of North China, which have been made by Professor Pumpelly and Baron Richthofen, are, I think, by no means unwarranted.

There can be no question but that the coal and iron of China will prove to be of immense value in the material development of the country so soon as she decides to adopt railways and foreign engineering methods."

On unpacking the collection I discovered that the plants were of Carboniferous age and that most of them belonged to species common in the rocks of Europe and North America.

Of the ten species which can be distinguished, one is a *Pecopteris* too imperfect for determination (probably *P. unita* Brgt.), and two others, a *Lonchopteris* and an *Archæopteris*, present slight differences from their closely-allied representatives in Europe and America. The other seven are undistinguishable from what may be considered as the most characteristic plants of our Coal-measures.

The complete list of species is as follows:

Annularia longifolia Brgt.
Sphenophyllum oblongifolium Germar.
Calamites Suckowii Brgt.
Cordaites borassifolius Ung.
Lepidodendron obovatum Sternb.
Sigillaria Brardii Brgt.
Pecopteris Cyathea Brgt.
Pecopteris, unita ? Brgt.
Archæopteris, n. sp.?
Lonchopteris, n. sp.?

The *Archæopteris* indicates that the coal with which these plants were associated belongs near the base of the Coal-measures, as this genus does not rise above that horizon.

The species of *Lonchopteris* and *Archæopteris* are best represented in the collection, and the former is very well shown. In general aspect it is not unlike the figures given by Brongniart of his *L. rugosa*, (Veg. Foss., p. 368, tab. 131, figs. 1, 2, 3), but the pinnules are smaller and the reticulation much more open. In the latter respect it is more like *L. Baurii* Andr., *L. Eschweilerianus* Andr., and *L. conjugata* Gœpp., sp. (*Neuropteris conjugata* Goepp.), but it has narrower, more pointed and curved pinnules than either.

Should a larger number of specimens show that these are constant characters it will be necessary to regard this as a new species which may be fittingly named after Mr. Hague, *Lonchopteris Hagueana*. The *Archæopteris* mentioned is a very graceful and well-marked species of the genus, having obovate or spatulate pinnules, of which the upper extremities are often crenulate or fimbriate. It is less robust than the type forms of *A. Hibernica* Forbes, sp., and the pinnules are more symmetrical. It is about the size of *A. Jacksoni* Dwn., but has less crowded, more elongate, and more regularly ovate or spatulate pinnules. The nerves are fine, forked only near the base, simple and sub-parallel above. Possibly this may be, like several described species of *Archæopteris*, only a variety of *A. Hibernica*, but the characters mentioned seem to separate it from any known form, and I would suggest for it the name *Archæopteris spatulata*.

The first fossil plants brought from China were collected by Professor Pumpelly, and were described by the writer in the Smithsonian Contributions of 1866. These included two new ferns, *Sphenopteris orientalis* N., and *Hymenophyllites tenellus* N., *Pecopteris Whitbyensis*, found in the Lias and Upper Trias of Europe, *Podozamites lanceolatus*, a Liassic plant of the old world, and *P. Emmonsi*, which occurs in North Carolina, all of which seem to represent the Upper Trias or Lower Lias.* Subsequently (in 1868), M. Ad. Brongniart exam-

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* My paper on the fossil plants collected by Professor Pumpelly has been reviewed by Heer (Juraflora Ostsiberiens. p. 17), and Schenk (Richthofen's China, vol. iv, p 264), with some suggestions in regard to their generic and specific relations which would hardly have been made had these distinguished paleontologists had access to the specimens on which I based my conclusions. Having examined these fossils I take occasion to offer here a few additional notes upon them.

Sphenopteris orientalis certainly belongs to the same genus with the ferns now called *Thyrsopteris Murrayana* and *Th. Maakiana* Heer, the latter being specifically hardly different from it. *Hymenophyllites tenellus* N., has finely-dissected pinnules, and is certainly distinct. My *Podozamites lanceolatus* is that plant and not a *Phœnicopsis* as suggested by Heer. This is shown by its nervation, and by the fact that the pinnules are pinnately set on a rachis and are not fasciculate as in

ined a collection of plants obtained from southern Shan
and gave a list of them in the *Bull. de la Soc. Geol.*
France, 3d Series, vol. ii, p. 408. They included son
of the species collected by Pumpelly, and were consider
by M. Brongniart to represent the Upper Trias and Low
Jura. More recently Baron v. Richthofen obtained fossil plan
from various parts of China, and these have been described l
A. Schenk in vol. iv of Richthofen's China. They repr
sent two distinct horizons, one Carboniferous and the oth
Mesozoic. The former were found in the districts of Shan
and Hunan. Here were obtained *Pecopteris cyathea*, *P. uni*
Annularia longifolia Brgt., *A. maxima* Schenk, *Sphenophyllu*
emarginatum Brgt., *S. Schlotheimii* Brgt., *Calamites gig*
Brgt., etc. Elsewhere in the provinces of Shansi and Tshi
Richthofen obtained a group of Mesozoic plants, among whic
M. Schenk recognized *Pecopteris Whitbyensis*, *Podozamites la*
ceolatus, and other species which led him to refer the stra
containing them to the Brown Jura.

It is known to most geologists that the extensive coal basi
of India, from which fossil plants have been described b
Oldham and Morris and Dr. Feistmantel, are all of Mesozo
age. The same is true of the coals of Tonking, Cochin Chin
from which a considerable number of fossil plants have bee
obtained by the French expeditions and described by M. I
Zeiler in the *Annales des Mines*, October, 1882.

It would seem proven, therefore, that the coal basins
China (in which the coal is very largely converted to anthracit
by local metamorphism), belong to two great geological sy
tems, one, as indicated by the plants collected by Baron Ric
thofen and Mr. Hague, the equivalent of the Coal-measures—an
probably the entire range of the Coal-measures of Europe an
America—the latter not yet capable of so definite classificatio
but probably referable to both the Rhætic and Lias.

The fossil plants brought by Mr. Hague from China, repr
senting as they do, not only the characteristic genera of th
Coal-measures in Europe and America, but identical
closely allied *species*, cannot fail to interest both geologists an
botanists; the first, by the confirmation they afford of the cla
sification adopted for the stratified rocks, based on the fossi
they contain; the latter, from the evidence they furnish

Phœnicopsis. *Taxites spatulatus* N. is the leaf of a conifer, and not of a cycad,
inferred by Heer. It has but a single nerve, the median, which is strong a
traverses its entire length, and has a wedge-shaped base terminating in a sho
twisted petiole. The publication of Heer's important paper on the Jura-flora
Eastern Siberia has given significance to certain specimens in Pumpelly's colle
tion and has enabled me to add to the list of species *Baiera angustiloba* He
(very near to *B. Munsterianum*), *Phœnicopsis longifolia* Heer, and *Czekanowsk*
rigida Heer.

the practical identity of the acrogenous flora of the Coal period over so large a portion of the earth's surface, and the remarkable persistence which specific characters exhibit in the wide range of migration and the incalculable lapse of time through which the dispersion was effected.

Since none of the higher plants were in existence upon the earth's surface during the Carboniferous age, wherever a terrestrial flora prevailed, it could only be composed of acrogens and gymnosperms, but how it happens that within these limits there was so little diversity is incomprehensible. During the Coal-measure epoch the same genera, and to a large degree the same species, seem to have lived in North America, Europe, Brazil and China.

No one who has any acquaintance with fossil plants would fail to recognize at once most of the species in the collection brought by Mr. Hague; but if shown seven out of the ten he could not say whether they came from America, Europe or Asia. And yet, in the interval between the deposition of the Coal-measures and the Triassic rocks the whole flora of the globe was revolutionized. Before the Bunter was laid down *Lepidodendron*, *Sigillaria*, *Annularia*, *Sphenophyllum*, *Cordaites*, and indeed all the characteristic forms of the coal flora had disappeared. The Cycads in great variety, true Equiseta, and peculiar genera of conifers and ferns, gave new aspects to nature, and this again over the whole world. From the desert of Atacama, from Sonora, New Mexico, North Carolina, Europe, India and China, we obtain the remains of the unmistakable Mesozoic flora with species which are common to all these widely separated localities.

Hence, we are safe in fixing by fossil plants the geological horizon of the Mesozoic coal basins of China; but the identity of species in the Mesozoic flora, though surprising, is not quite so marked as in that of the Carboniferous age.

In the Middle Cretaceous came another great revolution, and the angiosperms succeeded the gymnosperms so rapidly that the earth was covered with oaks, magnolias, willows, etc., before the Tertiary began. Gradually we are gathering the details of this wonderful history, and ultimately we shall be able to report the *facts* with a good degree of fulness; but the causes which inspired the revolutions that have taken place in plant life, and the processes by which these great changes have been effected seem to be as inscrutable as ever.

ART. XVII.—*Review of DeCandolle's Origin of Cultivated Plan[illegible]; with Annotations upon certain American Species;* by A[illegible] GRAY and J. HAMMOND TRUMBULL.

IN concluding, after some unexpected delay, our observations upon DeCandolle's *L'Origine des Plantes Cultivées*, it may not be improper to state that, as we learn, the work was planned upon a somewhat larger scale, which was reduced to suit the requirements of the publisher. This accounts for the omission of certain articles which would otherwise have found [illegible] place.

PART III.

Lycopersicum esculentum, Tomato.—We have only to note an oversight in respect to the Mexican cultivation of the *Mala Peruviana*, as it was named by some botanists of the 16th century. DeCandolle refers to Humboldt's statement that the cultivation of this esculent was ancient in Mexico, but adds that there is no mention of it in the earliest work on the plants of that country, viz: Hernandez, *Historia.* But Hernandez (ed. 1651, p. 295, ff.) actually has a chapter "De *Tomatl*, seu *planta acinosa* vel *Solano*, and describes several sorts under their Mexican names.*

Persea gratissima, Alligator Pear of the English, *l'Avocat* of the French; a singular corruption of a native name, as DeCandolle remarks, which had no more to do with an alligator than with a lawyer. Our author does not carry back the native Mexican name quite to its original, which was *Ahuacahuitl*, corrupted by the Spaniards into *Aquacate*, *Avogade*, etc. Champlain, who saw it in Mexico in 1599 or 1600, calls the fruit *Accoiates* and *Acoyates;* Voy. to the W. Indies [Hakluyt Soc., 1859], p. 28.

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* I find only *one* writer in the 16th century who gave the Tomato a name indicating a Peruvian origin—namely, Anguillara, whose treatise "De Simplicibus" was first printed, in Italian, at Venice, 1561. On his authority the name "*Poma Peruviana*" is introduced, in the synonymy, by C. Bauhin, in his annotations on Matthioli, 1598 (p. 761), and the *Hortus Eystettensis* (attributed to Besler), published in 1613, is referred to by the same writer (*Pinax*, 1671, p. 167) as authority for the names, *inter alia*, "Poma amoris fructu rubro & *Mala Peruviana*." The *Hortus Eystettensis* is the only authority cited for "*Mala Peruviana*" in the work to which M. DeCandolle refers—the *Historia Stirpium*, attributed to J. Bauhin, but published long after his death (in 1551), with large additions by his son-in-law Cherler, and by Chabraeus and Graffenreid. Guillandinus, of Padua, in a treatise "De Papyro," 1572, named the "*Tumatle Americanorum*," as a species of *Pomum Amoris* or *Solanum pomiferum*;" and, earlier, Matthioli had described it (*Comment. in Dioscor.*, ed. 1559, p. 537) as a "kind of Mala insana" [Solanum melongena], which was "beginning to be imported" into Italy, and which was "popularly called *Pomi d'oro*, that is, *Mala aurea*."

Anguillara may have confounded the Tomato with another of the American [illegible]naceæ, introduced at about the same period—the Thorn Apple (Datura Stra-[illegible]ium) which Guillandinus (1572) named "*Mala Peruviana*," and the French [illegible]d "*Pomme de Perou*." J. H. T.

Oviedo described "the wild pear-tree of the main land," in 1526. It grew "in the province of Castillo del Oro (Panama), in the sierras of Capira and the country of the cacique of Juanaga," etc. In the revision of his first work, in 1535, he adds, that he had, some years before, seen these trees cultivated by the Indians in Nicaragua (*Hist. gen. y nat.*, lib. ix, c. 22). It was still a tree of "Terra firma"—not yet introduced into the Islands. Clusius saw it in a garden in Valencia—"said to be brought from America"—thirty-five years earlier than the date (1601) mentioned by DeCandolle. He described the "Persea" in the *first* edition of his *Historia rariorum Stirpium*, 1576 (lib. i, c. 2), published five years after his journey in Spain.

Passiflora.—This genus is wholly omitted by DeCandolle; unaccountably so, considering how much Granadillas have been cultivated and prized in tropical countries. A note on the subject may not be out of place, as a species was cultivated by our own Indians.

As to early history and aboriginal nomenclature, Monardes (*De Simplicibus Medic.*, c. 66) says that the Granadilla was spontaneous in Peru, and that the fruit was highly esteemed by the Indians and by the Spaniards. Cieça de Leon (*Chron. del Peru*, c. 28), about 1550, saw it growing in the valley of the Rio Lile, near Cali (now in Colombia) and in the country about Pasto. The flower and fruit are mentioned by J. de Acosta (*Nat. and moral Hist. of the Indias*, b. iii, c. 28)—"the fruit sweet, and too sweet, in the opinion of some." Lery (1557–8) does not appear to have found it in Brazil, but it was common there before the middle of the 17th century. Piso and Marcgrav (*Hist. Nat. Brasil.*, 1648, pp. 106, 70) reckon nine species or more, of which four were cultivated—two, especially, for their fruits. The Tupi (Brazilian) name was *Mburucuià* (Montoya, 1639) which Piso and Marcgrav, *l. c.*, wrote *Murucuia;* the species which was generally cultivated for its fruit was *Mburucuia-guacu* (i. e. great *Murucuya*). Father R. Breton (*Diction. Caraibe*, 1665) gives *Merécoya* as the Carib name of the fruit; but this seems to have been adopted from the Tupi—for in that language *Mburucuîa* denotes the "fruit of a vine."

It is remarkable that the Tupi (and Carib) name went with the plant and its fruit to the country of the Algonkins, before the coming of Europeans. One species (*Passiflora incarnata*) was cultivated by the Indians of Virginia. "They plant also *Maracocks*, a wild fruit, like a lemon, which also increase infinitely" (Capt. John Smith, *Gen. Hist.*, p. 29); and, again (p. 25), Smith mentions the "fruit which the inhabitants call *Mara-*

cocks—pleasant, wholesome fruit"—among "things which are *naturally* in Virginia." Strachey (*Travaile into Virginia*, 72, 119) describes the "fruit called by the natives a *maracock*, which the Indians plant," etc.

Although no longer planted, the fruit of the spontaneous plant is still eaten in the southern Atlantic States; and its popular name, *May-pop*, is probably the last stage of the Tupi original.

Now our *Passiflora incarnata* is so like *P. edulis* (well known in cultivation), the home of which is in Brazil,* that botanists have been unable clearly to distinguish the two, except by the fact that ours, dying down to the ground at approach of winter, remains herbaceous. It occurs in a rather narrow geographical range; and Dr. Masters, in his elaborate study of the order (*Trans. Linn. Soc.*, xxvii, 641; see also *Flora Brasiliensis*), says that "being so far separate from the remainder of its allies of the same subgenus, [it] may be considered as an outlier." Altogether we may infer that the fruit and the name were originally derived from the same South American source.

Musa, *Banana*.—The author concludes, as did Robert Brown, that Banana and Plantain are varieties of one species; also that this species is of the Old World; that in all probability it was not known in the West Indies when discovered by Columbus; but that in respect to the western side of North America, there is some evidence which is not easily ruled out, especially the statement of Garcilasso that the Peruvians had the banana before the conquest, and of Stevenson ("Trav. in S. Amer.") who is said to have seen in the ancient Peruvian tombs beds made of Banana leaves. This is discredited because the author found beans in the same tombs, " et que la fève est certainement de l'ancien monde." But if Stevenson wrote *beans*, without doubt he meant the seeds of Phaseolus, not of Faba. It would rather seem that the Banana, like the Sweet Potato and Cocoa-nut had early been transported over the Pacific.

Phaseolus vulgaris, *Kidney Bean*.—Three weeks after his first landing in the new world Columbus saw, near Nuevitas in Cuba, fields planted with "*faxones* and *fabas* very different from those of Spain," and two days afterwards, following the north coast of Cuba, he again found "land well cultivated with these *fexoes* and *habas* much unlike ours." "*Faxones*" or "*fexoes*" were—as Navarrete notes, *Colec.* i, 200, 203,—"the same as *frejoles* or *judias*," Spanish names for kidney beans, which the Portuguese call *Feijaos*. Oviedo (1525–35) speaks of ie "*fésoles*, as the Spaniards call them, of which there are any kinds in the [West] Indias." These *fésoles*, he says (lib. i, c. 18) "are called by Pliny *fagivoles:* in Aragon we call them *dias*, and the seeds of those of Spain and of this country are operly the same." The natives of Hispaniola raise these *soles*, but they are much more abundant on the main land, pecially in New Spain and Nicaragua. "I have, in the ovince of Nagrando in Nicaragua, seen them gather a huned hanegas (bushels, nearly), of these *fésoles:* and they also, that country and other parts of that coast, have many other nds of *fésoles*, besides the common sort: some have yellow eds, others spotted," etc. In another place (lib. xi, c. 1), viedo, mentioning plants that had been brought from Spain Hispaniola and other parts of America, "in the beginning," imes "*Fésoles*, called in Aragon *Judias*, and in my country astile] *Arvejas luengas:*" but "of these, there is no need of inging more seed, for in this island and on the main many ushels are harvested every year, and in the province of Nicaraa they are indigenous (*naturales de la misma tierra*), and a reat number of bushels are produced yearly of these and of ther *fésoles* of other sorts and different colors," etc.

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* Not Mexico, although indeed said to have been brought from "New Spain" to the garden of the Farnese palace, in Rome, as early as 1619. It is described, under the name of *Maracot*—with excellent plates showing the plant, the flower, and the fruit—by Tobias Aldinus, in *Rarior. Plant. Horti Farnesiani* (Romæ, 1625), [illegible] 49–59.

From this time (1535) onward, nearly every writer who menoned plants cultivated by the Indians named, together or in lose connection, maize, *beans* and pumpkins. Reference to everal of these writers has been made in our notes on *Cucuritæ*. Cabeça de Vaca found beans cultivated by the Indians f Florida in 1528, and again, near the western limit of his anderings (in New Mexico or Sonora) in 1535. De Soto, at is landing in 1539, found "fields of maize, *beans* and pumpins," near Tampa Bay; and at Coligoa (west of the Missisippi) "beans and pumpkins were in great plenty; both were arger and better than those of Spain;" and so, at other places, n his travel to the west and north.

Jacques Cartier, the discoverer of the St. Lawrence, on his irst voyage, 1534, found that the Indians near the mouth of hat river on the Bay of Gaspé had abundance of maize, and ad "beans (*febues*) which they name *Sahu*," or (as spelled in he vocabulary printed with his *Discours du Voyage*) *Sahe.*[*] The *Bref Recit* of his second voyage, 1535–6, mentions the use of corn and beans by the Indians of the St. Lawrence—"bled & *febues* & *poix*, desquels ilz ont assez" (f. 24).

Father Sagard in his History of Canada and in the account of his journey to the country of the Hurons, 1625, mentions the cultivation and use of "fezolles" by the Indians. The Hurons used in their succotash (*ñeintahouy*) "a third or a quarter part of their *fezoles*, called *ogaressa*" (Grand Voyage, 83, 138).

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[*] The language spoken by these Indians was a dialect of the Huron-Iroquois group, and we trace the name *sahe* (as Cartier caught it) in the Mohawk *osahe-ta* "féoles" of Bruyas (17th century) and the Onondaga *ousahèta* and *hôsahèta* "poix, féve" (Shea's *Onondaga Dictionary*).

Lescarbot, 1608, says that the Indians of Maine, like those of Virginia and Florida, plant their corn in hills, "and between the kernels of corn, they plant beans marked (*fèves riolées*) with various colors, which are very delicate; these, because they are not so high as the corn, grow very well among it" (*Hist. Nouv. France*, ed. 1612, p. 835; see also, p. 744).

The relation of the voyage of Captains Amidas and Barlow to Virginia, 1584, mentions *pease*, melons, etc., at Roanoke Island, but does not name beans; but Harriot, who accompanied them on this voyage, includes both "*Wickonzour*, called by us Pease," and "*Okindjier*, called by us Beans," among the productions of that country. Capt. John Smith, who was in Virginia in 1607, and Strachey, who was there in 1610, describe (in nearly the same words) the Indian manner of planting corn and beans: "they plant also pease they call *assentamens*, which are the same they call in Italy *fagioli*: their beans are the same the Turks call *garvances*, but these they much esteem for dainties" (Smith's *Gen. Hist.*, 28; Strachey, *Trav. in Virginia*, 117). Evidently, these names are confounded. *Garvance* was the French name of the Chick Pea (*Cicer arietinum*), the Spanish *garbanzo;* and it is not probable that the "Turks" gave this name to any kind of beans; while *fagiuoli* was the Italian equivalent of Latin *phaseoli.* Strachey's Virginian vocabulary gives *assentamens* (and *otassentamens*) for "pease," and *peccatoas*, *peketawes*, for "beans."

It must be remembered that at the beginning of the 17th century, kidney beans—as well as vetchlings (*Lathyrus*)—were popularly regarded as a kind of pease, or "peason." Turner, in his *Names of Herbes*, 1548, says that "*Phasiolus*, otherwyse called *Dolichos*, may be called in English *long peasen* or *faselles;* . . . . in French *phaseoles*": and "*Smilax hortensis*, ... in French, as some wryte *Phaseole* . . . . may be called in English *Kydney beane*," etc., (*Engl. Dial. Soc.*, ed. 1881, p. 62, 74). Lyte's Dodoens, 1578, follows Turner for the English names of Phaseolus, "Kidney beane and Sperage; of some they are called Faselles, or Long Peason," etc. (p. 474): his "common Peason" and "middle Peason" are *Ervilia* (*Ervum Ervilia* L.?) and *Pisum arvense* L.; while *P. sativum* is distinguished as "Great Peason, Garden Peason, and Branche Peason, because, as I thinke, they must be holpen or stayed up with branches" (*id.* 476).

So, on the continent, the Spanish names for "*fésoles*" was "*arvejas luengas*" (Oviedo) i. e., 'long vetches,' and the garden pea (*P. sativum*) was "un corto genero de Arvejas" (Calepin's

Diction., ed. 1616), a short kind of vetches. The confusion of names is frequent in writers of the 16th and first half of the 17th centuries, in French, as well as English and Spanish. "Magno sane labore"—as Tragus found—"*Phasioli*, *Orobi*, et *Pisa*, necnon *Cicer arietina*, maxime agrestia, secernuntur," *Stirp. Hist.*, 1552, p. 613.

Champlain uses *poix* and *fèves*, interchangeably, as names of the American *Phaseoli*. In Breton's *Diction. Fr.-Caraibe* (1666) the Caril name, *caláoüana*, stands for "pois de Brésil" and "febue de Brésil"; "pois rouges, dit Anglois, *mibipi*"; and "Pois, *mancônti*"—this being, probably, the (introduced) *P. sativum*.

Sagard (1624–5) says that the Hurons called the coarser part of their pounded maize—after the meal had been sifted from it—"*Acointa*, c'est à dire Pois (car ils lui donnent le mesme nom qu'à *nos pois*);" and in his *Dictionnaire Huronne*, he has "Pois, *Acointa*," "Fezolles, *Ogaressa*;" whence we infer that *French* pease [*P. sativum*] were already cultivated by or known to the Hurons. The Abnakis of western Maine, in the 17th century, called pease, *awennootsi-minar*, i. e. "French (or foreign) seeds." Tanner, 1830, gives as the Chippeway name of the "Wild pea vine" [Phaseolus diversifolius?] *Anishemin*, i. e. Indian (or, native) seeds.' In nearly all North American languages, the names for kidney-beans (*Phaseoli*) are of *earlier formation* than those for garden pease. The latter are usually formed *on* the former: e. g. Chahta, *tobi*, bean; *tobi hullo*, [wild] pea; *tobi hikint ŭni*, garden pea (Byington): Dakota, *o^{n}mnicha*, bean; *o^{n}mnicha hmiya^{n}ya^n* [i. e. 'round bean'], pea.

Without multiplying citations—we may assume that the "pease" and "poix" which early voyagers found cultivated by the American Indian were species of *Phaseolus*—not *Pisum*.

Five and twenty years before the settlement of Virginia, the Indians of Carolina and Florida had "fine citroüilles and very good *beans*" (Lescarbot, *Nouv. France*, 778). Lawson, 1700–1708, gives a more particular description of the southern beans cultivated by the Indians: "The *Kidney-Beans*," he says, "were here *before the English came*, being very plentiful in Indian cornfields." "The *Bushel* bean," a spontaneous growth, very flat, white, and mottled with a purple figure, was trained on poles [*P. multiflorus?*]: "Indian *Rounceval*, or Miraculous Pulse, so called from their long pods and great increase; they are very good, and so are the *Bonavies*, *Calavancies* [= *Garvances?*], *Nanticokes*, and abundance of other pulse, too tedious too mention, *which we found the Indians possessed of when first we settled in America*" (*Voyage to Carolina*, pp. 76, 77).

In the northern States, we find little difficulty in establishing the identity of *Phaseolus vulgaris* with the beans cultivated by the Indians at the first coming of Europeans. These were, from the first, distinguished, as "*Indian* beans," from the garden beans (*Vicia Faba*) introduced by the English. In 1609, Hudson, exploring the river which bears his name, saw at an Indian village—in the vicinity of Schodac and Castleton, Rensellaer county, N. Y.—"a great quantity of maize or Indian corn, and *beans* of the last year's growth" (Hudson's *Journal*, in De Laet, 1625, b. iii. ch. 10, and Juet's, in Purchas: *N. Y. Hist. Soc. Coll.*, 2 Ser., i. 300, 325).

1631–42. The Indians of New Netherland "make use of *French beans* of different colours, which they plant among their maize. . . . The maize stalks serve, instead of the poles which we use in our Fatherland, for the beans to grow upon" (De Vries, *Voyages*, transl. in 2 *N. Y. Hist. Soc.*, iii. 107).

1653. Van der Donck, in his "Description of the New Netherlands," distinguishes the beans cultivated by the Indians before the coming of the Dutch, and the *Turkish beans* which had been introduced: "Of Beans there are several kinds; but the large Windsor bean [*Vicia Faba*] . . . and the horse bean will not fill out their pods. . . . The Turkish beans which our people have introduced there grow wonderfully. . . . *Before the arrival of the Netherlanders* [1614] *the Indians raised beans of various kinds and colours*, but generally too coarse to be eaten green or to be pickled, *except the blue sort*, which are abundant," etc. He then describes the Indian mode of planting beans with maize, *ut supra* (*N. Y. Hist. Soc. Coll.*, 2 Ser., i. 188–9). This is the only reference we have found to the *introduction* of any species of *Phaseolus*, into North America. Van der Donck's book was written more than forty years after Hudson's coming, and the author first arrived in New Netherland in 1642. His statement as to the introduction by the Dutch of the best kind of "Turkish beans" for "snaps," salad, or pickling, is not to be accepted without reserve; but the fact that Turkish beans "grow wonderfully, fill out remarkably well, and are much cultivated," while the imported Windsor beans [*Vicia Faba*] and horse-beans proved failures is to be noted.

Wood, who was in Massachusetts from 1629 to 1633, says that the Indians, "in winter-time have all manner of fowles, *Indian beanes*, and clams" (*N. E. Prospect*, pt. 2, ch. 6). Roger Williams, 1643, gives the Indian name of these beans, in the Narraganset dialect: *Manusqussed-ash* (plural); Cotton's Massachusetts vocabulary (1727–8) has (sing.) "*Mônasquisset*, an Indian bean;" President Stiles, about 1760, heard the name in the Pequot dialect as *Mushquissedes* (MS. Vocab.); Zeisberger, 1776 and 1803, wrote it in the Delaware, with dialectic modification, *Malachxit;* and we can trace it in the modern Shyenne *Mónisk*

(Hayden's Vocab., 1862) and *Monchka*. In the Chippeway, the kidney-bean has received—probably from some local variety—a different name: *Miskodissimin*, i. e. 'red-dyed seed (or fruit);" and this name, modified as *M'skochī-tha*, was used by the Shawanoes of Ohio.

To return to New England, Josselyn, who was in this country, 1638–9, and again, 1663–71, in his catalogue of "plants proper to the country, names "*Indian Beans*, falsely called *French* beans:" "the herbalists call them kidney-beans, from their shape and effects. . . . They are variegated much [in size and colour]; besides your *Bonivis* and *Calavances*, and *the kidney-bean that is proper to Roanoke:* but these are brought into the country: the others are *natural to the climate*" (*N. E. Rarities*, p. 56; *Voyages*, p. 73–4). Here is reference to at least *two* species of American beans, one "proper to New England," the other from Roanoke—perhaps *P. multiflorus*.

Besides the names already mentioned—*Mônasquisset*, with its variants—there is another, in northern Algonkin languages, for kidney-beans, which must have originally belonged to some *high-twining* variety. Eliot used it, in the plural, for "beans" in 2 Samuel, xvii. 28, *tuppŭhquam-ash*—which literally signifies 'twiners;' and Rasles (1691–1700) gave, in the Kennebec-Abnaki of Maine, for "faséole, *a'teba'kwé*—from the same root. A modern Abnaki vocabulary shows that this name is still in use—as "*ad-ba-kwa*."

As to the American origin of *P. lunatus*, the Lima bean and its varieties, there seems now to be no question. "It is evidently," says M. DeCandolle (p. 276), "a Brazilian species, dispersed by cultivation, and perhaps long ago naturalized, here and there, in tropical America."

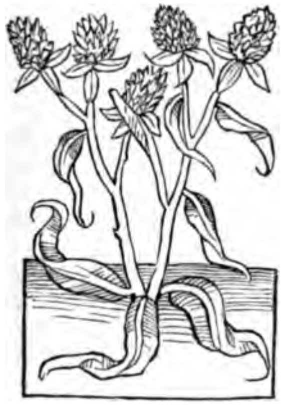
But as to the origin of *P. vulgaris* he is not free from doubt. He finds "(1) that this species was not cultivated in ancient times in the East Indies, the southwest of Asia, or Egypt; (2) that we are not absolutely certain that it was known in Europe before the discovery of America; (3) that at that epoch the number of varieties suddenly increased in the gardens of Europe, and all authors began to speak of it; (4) that the majority of species of this genus are found in South America; and (5) that seeds which apparently belong to this species have been found in Peruvian tombs [at Ancon] of a date somewhat uncertain, mixed with many other species, all of which are American" (p. 275).

The proof that *P. vulgaris* (and *P. nanus*), in varieties almost innumerable, were cultivated by the natives of America, before the coming of Europeans, seems to be conclusive. The resolution of M. de Candolle's doubts as to the American origin of the species must depend, chiefly, on the identification of the species known as *Phaseoli* (*Phaseli*, *Fagiuoli*, *Fésoles*, etc.), in

Europe, *before* the discovery of America. This identification may not be impossible, but the space at our disposal will not permit us to attempt it in this article, or even to re-examine the authorities on which M. de Candolle admits the probability—"that the *Dolichos* of Theophrastus was our pole bean (*haricot à rames*), and the *Fasiolos* our cultivated bush bean (*haricot nain*"), p. 271. At present, we have only to offer one or two notes—

1. The distinction indicated by Galen (*de Alimentis*, lib. i., cc. 25, 28), between the *Phasiolos* (φασίολος), of Dioscorides and *Phaselus* (φάσηλος)—presumably the "vilis faselus" of Virgil—if well founded, seems to have been lost sight of in the middle ages. In Italy, the Greek and Latin names *Phasiolos*, *Faseolus*, *Faselus*, *Fasillus*, etc., passed into the modern *Fagiuoli*. Piero de' Crescenzi, of Bologna, whose treatise on agriculture was written near the beginning of the 14th century, in Latin, and translated into Italian about 1350, mentions, among field plants *Faseoli* (*Fagiuoli*), as well known "some of them are red, some white .... They are planted, conveniently among panick, millet and chick pease; they are also planted in gardens, among cabbages and onions."* It is not certain that the red and the white were of the same species, or genus, or that either was a species of *Phaseolus* L. In the first half of the 16th century the white Phaseoli were the more common and less esteemed. The young and tender pods were eaten, with the included seeds, in salads, or boiled with other vegetables.

1.



Two other early figures show that the *Faseoli* were not so "well known" to the herbalists of the 15th and beginning of the 16th century as to Crescenzi in the 14th. One (fig. 2) is from a Venice edition of the *Hortus Sanitatis*, 1511; the other (fig. 3) from the *Tacuini Sanitatis* of "Elluchasem Elimithar," Strasburg, 1531, p. 49. They are equally unlike the modern *Phaseolus*, the earlier figure in Crescenzi, and each other. Fig.

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* *De Agricultura*, lib. iii, c, 10 (Italian, Ed. Venice, 1504). The Latin text was first printed at Strasburg, in 1471, and with *figures*, 1486; the Italian version was printed at Florence, 1478. The figure (fig. 1) of *Faseolus* in the earliest (Latin) edition we have seen, without date, but probably of Louvain, about 1480, has little resemblance to the *Phaseolus* of modern botany.

M. de Candolle remarks (p. 272) that "authors of the 15th century say nothing of *Faseolus*, or any analogous name," and that "this is the case with P. Crescenzi,"—referring to a French translation of Crescenzi, printed in 1539, which we have not seen.

3 may have originally been intended for a Teasel (*Dipsacus sylvestris*) the *Virga pastoris* of the herbalists. Calepin's Dictionary (ed. 1616) says, s. v. *Faseolus*, that the name *Fasilli* is now given by the common people, to "a species of *Cicercula*."

2.

3.

This "vulgaris Phaseolus" of Matthioli* and other Italian botanists of the period, is figured and described in the later editions of his commentary on Dioscorides,† as having an erect stem, ternate leaves, white flowers, kidney-shaped seeds, white, "*except the umbilicus, which grows black.*" It may be worth noting that "a black spot in the place of the cotyledon" was a characteristic of the *Faseoli* described by Albertus Magnus (13th cent.), which appears to M. de Candolle "to be the dwarf Haricot (*P. nanus*) of our epoch." This black spot is more strongly suggestive of *Dolichos* than of any known variety of *P. vulgaris*: e. g., *Dolichos unguiculatus* L. (French, *D. Mongette*, *Banette*, *Haricot cornille*)—not mentioned by M. de Candolle, but much cultivated in Italy, and of which there are a great number of varieties—which has seeds "marked by a prominent black spot, about the umbilicus."‡

2. M. de Candolle (p. 272), with a reference to Delile and to Piddington's *Index*, remarks that though "no Hebrew name corresponding to the *Dolichos* or *Phaseolus* of the botanists" is known, yet "a name less ancient, because it is *Arabic*, namely *Loubia*, is found in Egypt for the *Dolichos Lubia*; and in

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* "Vulgares Phaseoli, quibus passim in cibis vescimur, dum satis in campis virent, *non repant*," etc., Matth. *Apologia adv. Amathum*, 1559, p. 33; "Vulgaris usus Phaseolus." *Ibid.* 31.

† *Ed.* C. Bauhin, 1598, p. 341. In the earlier edition (*Venice*, 1559, p. 264), *Phaseolus* is figured as a low, bushy and spreading, but not twining plant.

‡ Several other species of *Dolichos* (e. g., *D. sesquipedalis* L., Ital. *Fagiuolo Sparagio*, Engl. Asparagus bean) are similarly marked. Vilmorin-Andrieux et Cie, *Les Plantes Potagères*, 1883, p. 280. Other names for this species are: "*Germ.* Ostindische Riesel-Spargel Bohne, Nagelische Fasel; *Ital.* Fagiuolo dell' occhio; *Span.* Garrubia, Moncheta, *Judia* de Careta."

Hindustani, under the form *Loba*, for *Phaseolus vulgaris.*" This name seems to be clearly referable to the Greek. It has not been traced earlier than to Jahia ebn Serapion—an Arabian physician of the 9th or 10th century—whose work "de Simplicibus" compiled chiefly from Dioscorides and Galen, was translated into Latin in the 15th century.* In a chapter (lxxxi) on "*Lubia*, i. e., Faseoli," he quotes from Dioscorides, the description of *Smilax hortensis* (*κηπαία σμίλαξ*) "whose seeds some call *Lobia*"; and it is evident that the name *Lubia* (as it was transliterated from.the Arabic text by the translator) was *transferred to the Arabic* from the Greek of Dioscorides. It is probable, to say the least, that it has been rightly appropriated to *Dolichos Lubia* Forskal (De Candolle, 278), rather than to any species of *Phaseolus.*

The length to which our annotations have extended forbids all notice of the third part of this book. This, however, is very brief. It contains a tabulation of the plants of cultivation, and of the results of the preceding discussion of them; also an article on the regions in which the principal species have originated or have been brought into cultivation, in which it is stated that of the 247 species under investigation the Old World has furnished 199, and America 45, leaving three which are doubtful in this regard; and the extreme poverty of the southern hemisphere beyond the tropics is a striking feature. An article on the number and nature of species cultivated at different periods is noteworthy. So, also, is the enumeration of the cultivated plants which are unknown in a wild state; from which it is gathered that 27 species have never been found wild by any botanist, 27 more are doubtful in this respect, while 193 are of recognizable origin.

Of the "Reflexions diverses," at the close, we note only the final one, that "In the history of cultivated plants, I have found no indication of communications between the inhabitants of the Old and New World anterior to the discovery of America by Columbus. The Scandinavians, who had carried their expeditions to the northern United States, and the Basques of the Middle Ages, who had extended their whaling voyages perhaps to America, would appear not to have transported a single cultivated species. The Gulf-stream has equally been without effect. Between America and Asia two transportations may have been effected. one by man (the *Batatas*), the other either by man or by the sea (Cocoa-nut)."

Perhaps the Banana should be ranked with the Sweet Potato in this regard. And we may merely conjecture that the Purslain came to our eastern coast with the Scandinavians or the Basques.

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* Milan. 1473. and Venice. 1479; but better known to botanists of the 16th century in the Strasburg edition of 1531, edited by Otho Brunfels.

ART. XVIII.—*On the Supposed Human Foot-prints recently found in Nevada;* by O. C. MARSH.*

DURING the past summer, various accounts have been published of the discovery of human foot-prints in sandstone near Carson, Nevada. The locality is in the yard of the State prison, and the tracks were uncovered in quarrying stone for building purposes. Many different kinds of tracks were found, some of which were made by an animal allied to the elephant; some resembled those of the horse and the deer; others were apparently made by a wolf. There were also tracks made by large birds.

1.

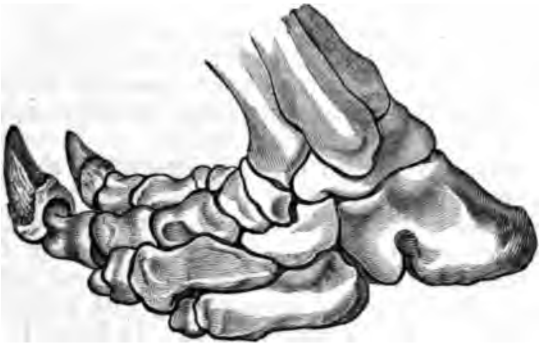

Figure 1.—Left hind foot of *Mylodon robustus* (after Owen). One-sixth natural size.

2.

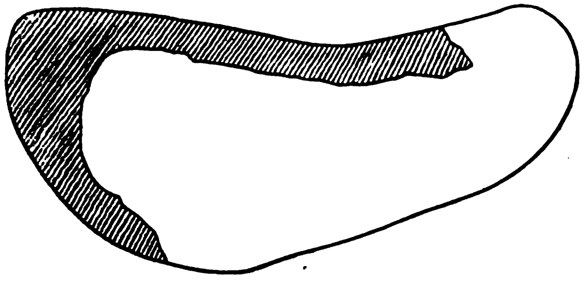

Figure 2.—Left foot-print at Carson (after Harkness). One-sixth natural size.

The foot-prints occur in series, and are all nearly in the same horizon. Some of the smaller tracks are sharp and distinct, but most of the impressions are indefinite in outline, owing apparently to the fact that the exact surface on which they were made is not usually exposed.

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[*] Abstract of a paper read before the National Academy of Sciences, at New York, November 17th, 1882.

The supposed human foot-prints are in six series, each with alternate right and left tracks. The stride is from two and one-half to over three feet in extent. The individual foot-prints are from eighteen to twenty inches in length, and about eight inches wide. The distance between the line of right hand and left hand tracks, or the straddle, is eighteen to nineteen inches.

The form and general appearance of the supposed human tracks is shown in figure 2, which is a reduced copy of one of the impressions represented by Dr. W. H. Harkness, in his paper before the California Academy of Sciences, August 7th, 1882. The shaded portion was restored by him from other foot-prints of the series. A copy of this impression was given, also, by Professor Joseph LeConte, in his paper before the same society, August 27th, 1882.

The size of these foot-prints, and especially the width between the right and left series are strong evidence that they were not made by men, as has been so generally supposed.

A more probable explanation is that the impressions are the tracks of a large Sloth, either *Mylodon* or *Morotherium*, remains of which have been found in essentially the same horizon. In support of this view it may be said that the foot-prints are almost exactly what these animals would make, if the hind feet covered the impressions of those in front. In size, in stride, and in width between the right and left series of impressions, the foot-prints agree closely with what we should expect *Mylodon* or *Morotherium* to make. In figure 1, the bones of the left hind foot of a species of *Mylodon* are represented, the figure being reduced to the same scale as the accompanying cut, figure 2, of one of the supposed human foot-prints.

The geological horizon of these interesting foot-prints is near the junction of the Pliocene and Quaternary. The evidence, at present, appears to point to the Equus beds of the upper Pliocene as the nearest equivalent.

Since the above communication was read, the writer has had an opportunity of examining photographs and casts of the Carson foot-prints, and is confirmed in his opinion that the supposed human tracks were made by large Edentates. The important fact has recently been determined that some of these tracks show impressions of the fore feet. The latter are somewhat outside of the large foot-prints, as would naturally be the case, if the animal changed its course.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Contributions from the Chemical Laboratory of Harvard College during the Academical Year* 1882–83.—Professor C. LORING JACKSON and Mr. A. E. MENKE in continuation of their work on turmeric have prepared the mono and di acetcurcumin, nd shown that the rich purple substance formed by the action f phosphoric oxychloride on curcumin has a composition which iffers at most but very slightly from curcumin and is probably n anhydride formed by removing one molecule of water from wo of curcumin. By oxidizing turmerol with a solution of otassic permanganate they have obtained two acids, $C_{11}H_{14}O_2$ nd a second a melting at 221° C. which has probably the compo-tion $C_{10}H_{10}O_4$. The first of these acids throws some light on the osition of the hydroxyl group in the molecule of turmerol.

Professor Jackson and his assistant have also studied the action f phosphorous chloride on aniline and have shown that the pro-uct first formed is probably $C_6H_5NHPCl_2$, although they have not ucceeded in isolating this body. Upon heating the crude pro-duct a material was obtained which yielded on treatment with alcohol $(C_6H_5NH)_2POH$. This last substance has been studied carefully and its composition indicates that the intermediate pro-duct was $(C_6H_5NH)_2PCl$.

An investigation of the constitution of camphor undertaken by the same chemists failed so far as its main object was concerned, but it led them to a method of preparing borneol from camphor much simpler, easier and giving a larger yield than either of those now in use. This method consists in dissolving camphor n common alcohol and adding sodium. One crystallization from alcohol is sufficient to give pure borneol, and the yield is essen-tially that demanded by theory.

Professor JACKSON and Mr. G. T. HARTSHORN have studied the sulphur derivatives of parabrombenzyl, including the sulphide, sulphone, mercaptan, mercaptide and disulphide of this radical; as well as the potassium, calcium, barium and lead salts of the corresponding sulpho-acid and also the chloride of its radical. Descriptions and analyses of these several products will be found n a paper recently published by these authors in the Proceedings f the American Academy of Arts and Sciences.

In order to obtain data for determining the constitution of pyro-mucic acid, Professor H. B. HILL has begun the study of its sub-itution products. Thus far he has examined only a monobrom-pyromucic acid melting at 184°–185° C. While under certain onditions oxidizing agents, such as nitric acid or bromine ater, convert this substance into fumaric and carbonic acids, it so appears that under slightly different conditions the action bromine leads to the formation of dibromsuccinic acid and odibromsuccinic acid, besides a neutral body, dibromfurfurantetrabromide, which when treated with the alcoholic potash gives tetrabromfurfuran. This result is especially interesting because these two last bodies are derivatives of Limpricht's so-called tetraphenol.

Professor Hill with Mr. E. K. **Stevens** has also studied the action of alkalies upon mucobromic acid. Professor Hill, in connection with Mr. O. R. **Jackson**, had previously found that a larg excess of an alkaline hydrate decomposed mucobromic acid int formic and dibromacrylic acids, the last passing also into brompropiolic acid. Quite a different reaction ensues if care is take to keep the solution barely alkaline. There is then formed th salt of a dibasic acid, which is so unstable that it cannot be isolated. The analyses of its salts show that this acid is forme from mucobromic acid by the replacement of Br by OH, an it is therefore isomeric with brommaleic acid. Both the fre acid and its salts give a deep purple red coloration with ferri chloride and reduce argentic oxide when warmed in contac with this reagent. Bromine water at 100° C. gives oxalic acid and bromal hydrate. Alkalies in the cold produce no furthe change, but on boiling oxalic and carbonic acids are formed. Professor Hill and Mr. Stevens do not feel warranted at present in giving a definite structure formula to the new acid. They have tried without success to obtain an analogous product by using sodic ethylate. Potassic phenylate, however, reacts readily on mucobromic acid, and replaces bromine by the phenoxy group. But this phenoxy compound shows more analogy to mucobromic acid itself than to the hydroxyl derivative just described. With alkalies it is converted into formic acid and phenoxybromacrylic acid. This last acid shows great stability and the circumstance that the Br atoms can neither be replaced or split off as HBr, indicates its constitution.

Professor Hill and Mr. **Morris Loeb** have made from mucochloric acid the corresponding phenoxychloracrylic acid.

Dr. C. F. **Mabery** obtained γ dichlordibrompropionic acid by the addition of chlorine to β dibromacrylic acid. By the action of a solution of baric hydrate, the elements of hydrobromic acid were eliminated from the γ acid and a dichlordibromacrylic acid was formed. Two other dichlordibromacrylic acids were prepared b the action of baric hydrate on the α and β dichlordibrompropionic acids respectively. By the addition of chlorine to brompropiolic acid a bromtetrachlorpropionic acid was formed. Chlorine was also taken up by chlorbromacrylic acid; and from th resulting trichlorbrompropionic acid, baric hydrate abstracte the elements of hydrobromic acid, forming trichloracrylic aci With chloride of bromine an addition-product of brompropioli acid was obtained, a solution of brompropiolic acid in chlorofor absorbing chloride of bromine to nearly the extent required b theory. In order to determine the structure of the resultin chlordibromacrylic acid the elements of hydrobromic acid we removed from the chlortribrompropionic acid by the actic of baric hydrate; but whether the chlordibromacrylic acid thus obtained is identical or isomeric with the other remains to be determined. The study of the acids thus obtained by Dr. Mabery is still in progress.

Dr. L. P. **Kinnicutt** and Mr. G. M. **Palmer** have studied the β phenyltribrompropionic acid. This acid was obtained by the addition of dry bromine to the β bromcinnamic acid (melting point 120° C.) The substance is soluble in alcohol, ether and chloroform. By recrystallization from chloroform it can be easily purified, and when so obtained melts at 151° C.) It is decomposed by boiling water, giving the α bromcinnamic acid (melting point 132° C.); together with a new acid, very soluble in water and melting at 184° C., which is beyond doubt a phenyldibromlactic acid, and an oil which boils with slight decomposition at 253°–255° C. This oil is a dibromstyrol which when exposed to bromine vapor forms an addition-product. This addition-product, itself a very thick oil, is probably a tetrabromstyrol, and we expect that the study of the products formed when this last body is treated with boiling water, or with alkalies, will show the constitution of the dibromstyrol, and by inference that of the monobromcinnamic acid whose melting point is 120° C. and which is commonly called the β acid, although the position of the bromine in the molecule has never been satisfactorily determined.

In the course of his work Dr. Kinnicutt has devised a method of preparing carbonic oxide gas (CO) which is a valuable addition to our methods of preparing this well known aeriform substance, and forms a striking lecture-room experiment. Dr. E. Noack has lately shown that carbonic oxide can be obtained by passing carbonic dioxide over heated zinc-dust. His results suggested the idea that the same gas might be more conveniently made by heating carbonates directly with zinc dust, and it was found that when pulverized magnesite, mixed with twice its weight of zinc dust, was heated in a copper retort, a rapid evolution of carbonic oxide gas took place, which after the first few minutes came over very nearly pure, containing less than one per cent of carbonic dioxide. The magnesite used was that from Eubœa in Greece, which is imported here for making artificial stone, but any form of anhydrous magnesic carbonate would serve as well.

Mr. **John N. Nef**, of the Junior class, has made, under the direction of Dr. Kinnicutt, some experiments on the determination of nitrous acid by potassic permanganate. The determination of nitrous acid by adding a very dilute solution of the acid to a weak solution of potassic permanganate often proves troublesome and inaccurate, owing most probably to the slow oxidation of the last traces of nitrous acids. It was found, however, that very accurate results could be obtained by the following modification the process. To a solution of a nitrite in water, two or three ops of dilute sulphuric acid are added, then immediately an cess of potassic permanganate, the volume of the standard solun used being accurately measured. Next ten to fifteen cubic centimeters of pure H_2SO_4 previously diluted with its ow volume of water and cooled, are carefully poured in under co stant stirring, and the excess of potassic permanganate is the determined with a standard solution of oxalic acid. Five dete minations of a sample of potassic nitrite showed a variation o only $\frac{7}{100}$ of one per cent, and the same number of determinatio of sodic nitrite a variation of only $\frac{6}{100}$ of one per cent.

In a careful series of experiments, Mr. R. **Penrose**, of the Ju ior class, attempted to apply the same method, under Dr. Kim cutt's direction, to the estimation of the sulphites, but he did no obtain satisfactory results.

The Director of the Laboratory, Professor J. P. **Cooke**, ha continued his work on the Revision of the Atomic Weights. H has elaborated methods for determining with great precision th relations H: O: Ag; H: S: Ag; H: Cl: Ag.; H: I: Ag; H Br: Ag, fixing the relations between the members of each tria in a single series of determinations. Starting with the relatio Ag : Br = 108 : 80 in regard to which there is a universal agre ment he is seeking to fix the relations of each value at once t two others, whose mutual relations are already known, as in hi previous work on the relation,

$$Ag : Br : Sb = 108{\cdot}00 : 80{\cdot}00 : 120{\cdot}00.$$

In this way he hopes to be able to control so effectually th experimental errors that the questions in regard to the hypothesi of Prout or the constancy of the atomic weights may be finall settled. This investigation obviously involves a great amount o labor; and since it must be a long time before the final result can be reached this announcement is made to prevent an unneces sary duplication of the work. In connection with this investiga tion the writer has developed a simple method of correcting th weight of a body for the buoyancy of the atmosphere, which wa described in the previous number of this Journal, and similar sub ordinate results will be published from time to time as the inves tigation proceeds.

Under the direction of Professor Cooke, Mr. C. P. **Worcester** of the graduating class, has determined the vapor density of th chloride, the bromide and the iodide of antimony by means o the displacement process of Victor Meyer with the followin results, which are in each case the mean of three experiments:

	Density, Air=1.	Theory, SbR_3.
Antimonious chloride,	7·96	7·85
" bromide,	12·57	12·48
" iodide,	17·59	17·33

The first two values were obtained by displacement of air, th last by displacement of nitrogen, and the variations from theo do not appear to be greater than the errors inherent in the pr ess. It therefore appears that in the state of vapor the mol les of all three compounds have the same simple constitution. Mr. Worcester also determined the heat evolved in the famili reaction of zinc on dilute sulphuric acid. By experimenting with zinc dust, which dissolves with great rapidity when in contact with platinum, and previously determining the proportions of zinc and oxide of zinc mixed in the powder, it was found possible to conduct the reaction under such conditions that the heat evolved could be measured in a calorimeter. The values obtained were considerably greater than those calculated from the well-known heat of combination of sulphuric acid and zinc sulphate, and the results were interesting chiefly as controlling the more fundamental thermo-chemical data, and bringing to light discrepancies which must be reconciled before entire reliance can be placed on this new class of chemical constants. Investigations similar to this were undertaken by other students, but without equal success, and the results are reserved until sufficient data have been obtained to warrant positive conclusions.

Mr. S. H. KNIGHT, of the graduating class, re-determined the boiling point of sulphur by the method contrived by the writer, and previously described in this Journal by Mr. Z. R. Bennett. His experiments give as the mean value of many observations at the normal pressure of the air 447°·9, with a probable error of two-tenths of a degree, a value which is several degrees higher than that recently given by Professor Crafts. But although Mr. Knight's results agree very closely with that previously obtained by Mr. Bennett with the same method, it is not improbable that in both cases the boiling point may have been abnormally raised by the super-heating of the vapor, by impurities in the sulphur, or by the effect of the vessel used. The vessel, made of cast iron, was lined with a porcelain-like glaze, and directly heated with a gas flame.

Mr. OLIVER W. HUNTINGTON has determined the crystalline form of chlordibromacrylic acid, $C_3ClBr_2O_2H_2$, obtained as above by Dr. Mabery. The crystals are oblique prisms and have a well-marked triclinic habit. The prismatic axis was taken as the axis *z* and the axes of the only other zones capable of giving close measurements were taken as the axes of *x* and *y*. The elements of the crystal were calculated from the fundamental angles 010 on 100=66° 23′, 100 on 001=116° 17′, 010 on 001=116° 20′, 100 on 101=63° 50′, 100 on $1\bar{1}0$=47° 5′. The calculation gives

$$a:b:c: = 0{\cdot}79807:1:0{\cdot}72072$$
$$x \text{ on } y \quad 104^\circ\ 43'; \ y \text{ on } z \quad 71^\circ\ 6'; \ x \text{ on } y \quad 71^\circ\ 10'.$$

Besides the planes above enumerated the following were also determined:

$430;\ 0\bar{3}4;\ \bar{3}04?\ 7\bar{4}0;\ \bar{4}70;\ 4\bar{7}4;\ 047?$

Mr. Huntington also has had in preparation a collection of hemical tables, and of these a set of logarithms to five places of ecimals, arranged on an ingenious plan to facilitate rapid refernce, have been published.

2. *Electricity due to evaporation.*—In order to test the point hether the vapor arising from electrified surfaces of fluids is

positive, negative, or neutral, a number of exhaustive experiments have lately been undertaken in the Physical Laboratory at Berlin by Mr. Blake under the supervision of Helmholtz. It was found that the vapor arising from the electrified surface of water was neutral.—*Annalen der Physik und Chemie*, No. 7, 1883. J. T.

3. *Constant of Dielectricity.*—The electromagnetic theory of light is attracting so much attention that experiments upon electrical stress in dielectrics are becoming of great importance. Quincke has measured the constants of dielectricity and the double refraction of insulating fluids. Using Maxwell's equation,

$$\rho = \frac{K_1}{8\pi} \cdot \frac{P^2}{a^2}$$

in which ρ represents the stress along lines of electrical force, P denotes the difference of electric potentials, a the distance between the plane metallic electrodes between which the fluids are placed, K_1 the dielectricity constant, or the number which represents the increase of the capacity of a condenser when the plates are separated from one another by another insulating substance than air. According to Maxwell, K should be equal to the square of the index of refraction for light waves of infinite length. Quincke's results do not confirm this conclusion. A table of the constant for fifteen liquids is given. The double refraction of insulating fluids is also examined and is found to be variable. It is given in terms of a coefficient defined by Quincke. —*Phil. Mag.*, July, 1883, pp. 1–9. J. T.

4. *Conservation of Solar Energy.*—The June number of the Philosophical Magazine contains an article by E. H. Cook on Siemens's new theory, in which the author proves that CO_2 is distributed uniformly throughout our atmosphere, and concludes that the power of gaseous diffusion is such as to equalize the disproportion of gases which Siemens finds necessary for his hypothesis. Siemens in reply admits the uniform distribution of CO_2 in our atmosphere which is subjected to powerful circulating currents, but does not think that gaseous diffusion can work quickly enough to interfere with his photospheric currents. The effect of the slow action of gaseous diffusion he thinks would be inappreciable upon gaseous currents which arise from solar rotation, which amounts to 4500 miles an hour.

The metallic vapors in the photosphere are chiefly due to mechanical intermixtures. The metallic vapors thus introduced into the photospheric current will be burnt and projected outward into space, not as a vapor as supposed by Mr. Cook, but as a metallic dust much denser than the surrounding medium; the larger portion of which will according to Siemens return to the sun by virtue of solar gravitation and be dissociated on reaching the gaseous metallic sea underlying the photosphere. Siemens then aims to show that the light metallic vapors will be held in suspension in this metallic ocean in the same way that oxygen and carbonic anhydride are retained in sea water. In regard to Mr.

Cook's objection that the preponderance of hydrogen and CO in meteorites is inconsistent with the idea that the meteoric gases have been absorbed in space, Siemens remarks that it is sufficient to know that the meteorites contain all the constituents of our atmosphere except hydrogen: and the latter occurs in our atmosphere combined with oxygen; whereas in space and in the solar photosphere, we have evidence of its separate existence, or in combination with carbon. Mr. Cook's criticism upon the solar fan action is speedily dismissed by Siemens with the imputation that it is not understood by Mr. Cook. It is also pointed out that differences of combustion and richness of gaseous fuel may account for the light of colored stars. In conclusion Siemens refers to a recent paper by his brother, Dr. Werner Siemens, in which the latter cannot reconcile the nature and cause of the electric potential supposed to exist between the sun and the earth except by the adoption of Sir William Siemens' hypothesis. —*Phil. Mag.*, July, 1883, pp. 62–66. J. T.

5. *Metal Microphones in Vacuo.*—The action of the microphone is doubtless a complicated one, and experiments which tend to throw light upon the action between two surfaces in contact at the point where an electrical current passes are of much interest. Mr. J. Munro has constructed a microphone from two fine iron gratings or pieces of gauze which hang loosely against each other in a tube which has been exhausted as completely as possible. The contacts between the wire gratings could be modified by bringing a magnet to the exterior of the tube and thus lifting one grating more or less from the other. The glass tube was placed upon a sounding board and the ticking of a watch placed on this sounding board was very clearly heard in a telephone placed in the circuit. It was thought that the microphone in vacuo acted in an unusually perfect manner, and Mr. Munro thinks that this perfect action of the microphone could not have been supposed from certain experiments of Edlund on the polarization of electrodes in vacuum.—*Phil. Mag.*, July, 1883, pp. 23–25. J. T.

6. *Carbonic acid in air helps in condensing watery vapor.*—Professor Scacchi, in view of the production of clouds of vapor at the larger fumarole of the Solfatara of Pozzuoli by the burning of paper or branches of trees in the mouth (as shown by the guides to travelers), attributed the effect in 1849 to the carbonic acid produced by the combustion, comparing the action to that of hydrochloric acid which fumes in ordinary air. The question has recently been experimentally examined by Italo Giglioli of Portici. The conclusions of this author are as follows: (1) Carbon dioxide helps to condense watery vapor; (2) minute bodies suspended in the air are a powerful cause (the principal cause, as Coulier and Aitken have shown) in the condensation of aqueous vapor; (3) the effects at the fumarole must be ascribed to both the carbon dioxide and the minute carbonaceous particles set free during the combustion.—*Nature* of May 24.

7. *A Treatise on Electricity and Magnetism;* by E. MASCART and J. JOUBERT, translated by E. Atkinson. Volume I, general phenomena and theory. 654 pp., 8vo. London, 1883 (Thos. De La Rue & Co.)—Professor Atkinson has rendered an important service to English students in bringing before them this valuable volume by the French physicists MM. Mascart and Joubert. The entire work is to be embraced in two volumes, of which the first, now published, is theoretical, developing the general principles of the science from the mathematical side; the second is to be devoted to the description of the various phenomena, the methods of measurement and so on. The authors speak of the first volume, forming as it does, essentially, a distinct work, as an "Essay on the Mechanical Theory of Electricity," and this will give some idea of its scope. It is a work of high character, and should be carefully studied by every student of electricity.

II. GEOLOGY AND MINERALOGY.

1. *Elevated Coral Reefs of Cuba;* by W. O. CROSBY, (Proc. Boston Soc. Nat. Hist.)—Mr. Crosby describes, in this paper, the elevated coral reefs of Cuba, and draws from them the apparently well sustained conclusion that they indicate a slow subsidence during their formation, and hence, further, that Darwin's theory of the origin of coral islands is the true theory. The *lowest* reef-terrace, of the northern side of the island, has a height of thirty feet, and varies in width from a few rods to a mile; it was once, plainly, the fringing reef of the shore. The *second* reef-terrace rises abruptly from the level of the lower to a height of 200 to 250 feet, and bears evidence of having been of like origin with the lower. The altitude of the *third* reef is about 500 feet; and the *fourth* has a height east of Baracoa, near the Yumuri River, "of probably not less than 800 feet." These old reef-terraces extend "with slight interruptions—around the entire coast of Cuba; and in the western part of the island, where the erosion is less rapid than farther east, they are the predominant formation, and they are well preserved on the summits of the highest hills. Mr. Alexander Agassiz states that the hills about Havanna and Matanzas, which reach a height of over 200 feet, are entirely composed of reef-limestone."

In the precipitous mountain called El Yunque (the Anvil), five miles west of Baracoa, reef limestone, 1,000 feet thick, constitutes the upper half of the mountain, the lower part, on which the reef rests, consisting of eruptive rocks and slates; and originally the upper limit of this modern limestone formation must have been 2,000 feet above the sea-level. Mr. Sawkins gives 2,000 feet as the maximum thickness of the Jamaica elevated coral reefs above the sea.

Evidence that the reefs were not formed during a progressive rising of the land is drawn from the thickness of the reefs Mr. Crosby observes that the reefs reaching to a height of 500 and 1,000 feet—if not also that to a height of 2,000 feet show, by the remains within them, that they were made chiefly of reef-building corals, and hence, that they were not begun in deep water, as is assumed in the theory of Mr. Agassiz; but that they were made in shallow water during a progressive subsidence. Mr. Crosby concludes as follows:

"We have then apparently no recourse but to accept Darwin's theory, as an adequate explanation of the elevated reefs of the Greater Antilles; and therefore, to admit that the upheaval of this portion of the earth's crust has been interrupted by periods of profound subsidence during which the reefs were formed. The subsidence of 2,000 feet, of which El Yunque is a monument, must have reduced the Greater Antilles to a few lines of small, but high and ragged islands; and, as Mr. Bland has shown, this fully accounts for the absence in these immense tracts, of all large animals, although they were abundant here in Pliocene and earlier times."

The writer adds here the following objections to the theory of the formation of coral atolls in deep waters out of the calcareous secretions of deep-water life; (1) It is very improbable that submarine eruptions ever make the large and well defined craters, like those of subaerial action, which are appealed to in order to explain the lagoon feature of atolls; (2) many coral atolls are twenty miles or more in diameter, which is vastly larger than the largest of craters; (3) The atolls are never circular, and the larger have the irregularities of outline or diversities of form characterizing other large islands of the ocean; (4) In the actual reefs and islands of the Feejee group (see the map of the Islands in the writer's Corals and Coral Islands), all the conditions, from the first stage to that of the almost completed atoll, are well illustrated, one island having only a single peak of rock within the lagoon, not $\frac{1}{10}$th of the whole area, which a little more of subsidence would put beneath the waters and leave the lagoon wholly free. J. D. D.

2. *Glacier Motion.*—In a paper by W. R. Browne on glacier motion, read before the Royal Society in June, 1882 (and published in Nature, June 5), the author rejects all theories except that of Canon Moseley, which attributes it to change of temperature, and likens the motion to the creeping of a lead plate down a sloping surface. At the close of his paper the author publishes the following Greenland notes by Dr. Rae.

"When in Greenland, in the autumn of 1866, I was ice-bound at the head of one of the fiords, and slept a couple of nights at an Eskimo's house. A glacier about half a mile distant was then in full activity, the movement of which might, I believe, have been as visible to the eye as it certainly was audible to the ear.

My own idea is that Arctic glaciers must have a downward motion more or less during the whole year, summer and winter. I believe the alternation of heat and cold—or, I should rather say, of temperature—would of itself cause motion, especially near the upper surface.

We know that ice two or three feet or more thick contracts very considerably in a few hours by a sudden fall of fifteen or twenty degrees of temperature. I have found cracks in Lake Winnipeg three or four feet wide, formed by this cause during a single night, almost stopping our sledge journey. This gap soon freezes up. Then the weather gets milder, the ice expands, and with the new additional formation is too large for the lake, and is forced up into ridges. This process goes on at every 'cold snap,' alternating with milder weather. Now supposing a glacier for ten or more feet of its depth contracts by cold, as lake ice is known to do, it will get a series of cracks probably in its longest axis, say from inland seaward; the first snowdrift will fill up these cracks or some of them, and this filling up will to some extent perform the same office as the freezing of the cracks in the lakes. The longitudinal extent of the glacier will be increased. A snowstorm always brings milder weather, which would expand the glacier, but as this expansion would naturally tend downhill, instead of up, the whole motion would be downwards. But even if the cracks I mention did not take place, the contraction by cold would pull the ice downhill, not up, while the expansion by increase of temperature would tend to *push* the glacier downhill, so that these opposite actions would produce similar effects in moving the glacier, or such part of it as could be acted upon by external temperature, downwards.

I may also add that when a crack, however slight, is formed by contraction, the cold is admitted into the body of the glacier, and increases the contracting power or influence."

It will be remembered that Professor Agassiz's theory of glacier motion makes the dilatation arising from the freezing of the water that descends into the glacier the chief cause of motion. In his Geological Sketches, published in 1866 (p. 280), he states that other causes of motion have acted, but still urges that dilatation of the mass resulting from the freezing of infiltrated water was an important means.

3. *Glacial cold.*—In the Geological Magazine for July, Mr. S. V. Wood reviews briefly the facts with regard to the ice of the ice period, and the views as to glacial cold, and sustains the opinion he had previously presented that the period was "due neither to a change in the earth's axis, nor to any variation in the eccentricity of the earth's orbit, nor to any change in the distribution of land and water, but to a diminution in the heat-emitting power of the sun."

4. *Contributions to the History of Lake Bonneville;* by G. K. GILBERT. Ann. Rep. U. S. Geol. Survey, 1880-81, pp. 169-200, with plates and maps.—This paper is an abstract of Mr. Gilbert's full but yet unpublished Report on Lake Bonneville. Earlier contributions by him on the lake and its outlet are contained in volumes xv (1878) and xix (1880) of this Journal. The present paper relates prominently to climatic changes which the phases in the lake indicate; the inequality in change of level undergone

about the lake-area, which want of horizontality in the terraces evinces, and the orographic movements that occasioned it; and the relations in time to periods of volcanic activity at points over the region. Mr. Gilbert observes that the highest water-line about Lake Bonneville (the flooded Great Salt Lake and other Utah lakes combined) is 1000 feet above the level of Great Salt Lake; and "over every foot of the intervening profile can be traced evidence of the action of waves." This 1000-foot terrace is called the Bonneville shore-line; and another, which is especially conspicuous, at a level of 400 feet, is called the Provo shore-line. The remarkable terraced features of the region, which skirt all the lower slopes of the mountains, are well shown in plates accompanying the memoir.

The orographic movements about the great lake, according to careful measurements of heights, had their axis near the 113th meridian, the altitudes along the eastern and western shores being about 100 feet below those at intermediate points (at Promontory and Grantsville); and at the extreme southern limit, the southwestern shore of Escalante Bay, the rise in the surface westward was 200 feet in about 25 miles.

Mr. Gilbert deduces from the observations that an increase in elevation of the Wasatch Mountains may be still in progress. A cut represents the great faulting of the strata along the west side of this mountain region, separating its upturned rocks from those that remained nearly horizontal farther west, and states that, in an escarpment showing this line of fault, the time of the latest movement is indicated by the character of the erosion; that the eroded slopes are not a result of subaerial action, but are characterized by the peculiar sculpture of waves; that the last uplift of the Wasatch Mountains along the fault took place after the era of Lake Bonneville and the formation of the Bonneville and Provo shores; and also that the waters of the Great Salt Lake have not since been even 50 feet higher than they are at present. He adds that this uplift is therefore so recent "as to leave no reasonable suspicion that its growth has now ceased."

The memoir closes with the following "summary:"

(1) The climatic episode of which Lake Bonneville was the expression consisted of two humid maxima, separated by an interval of extreme aridity. The second maximum was the more pronounced; the first the longer.

(2) The time elapsed since the close of the Bonneville epoch has been briefer than the epoch, and the two together are incomparably briefer than such a geologic period as the Tertiary.

(3) The period of volcanic activity in the Great Basin, which covered a large share of Tertiary time, continued through the Quaternary also, and presumably has not yet ended.

(4) Such earth movements as are concerned in the molding of continents had not ceased in Western Utah at the close of the Bonneville Epoch, and presumably have not yet ceased.

(5) The Wasatch Range, the greatest mountain mass of Utah, has recently increased in height, and presumably is still growing.

5. *China*; by **Ferdinand Freiherrn von Richthofen.** Vol. IV. Berlin, 1883.—The fourth volume of Baron von Richthofen's great work on China has just appeared, and is published in the same luxurious style with its predecessors. It forms the paleontological part of the work and consists of descriptions and discussions of the fossils obtained from different parts of China, contributed by various authors: 1, Cambrian trilobites, by Wilhelm Dames; 2, Cambrian brachiopods, by Emanuel Kayser; 3, Middle and Upper Silurian fossils, by the same author; 4, Upper Silurian corals, by Lindström; 5, Devonian fossils of southwestern China, by E. Kayser; 6, Devonian and Carboniferous fossils of Tshau-tiën, by E. Kayser; 7, Carboniferous Foraminifera of China and Japan, by Conrad Schwager; 8, Upper Carboniferous fauna of Lo ping, by E. Kayser; 9, Carboniferous plants, 10, Jurassic plants, 11, Tertiary plants of Southern China all by A. Schenk.

These memoirs are illustrated by 54 well-executed plates.

In looking through this volume no one will fail to be impressed with the remarkable resemblance and to a large degree identity of the fossils brought by Richthofen from China with those which are most characteristic of the formations holding the same relative positions in Europe and America. Very few new genera are described and a large majority of the species are identical with those previously made known by Hall, Meek, Goldfuss, Römer, Sowerby, Dalman, Angelin, Davidson, Brongniart, Göppert, Heer and others. For example: in twelve species of *Productus*, enumerated as occurring in the Chinese Carboniferous rocks, only one is described by Kayser as distinctly new. The Carboniferous and Devonian brachiopods form the largest group of well-preserved fossils represented, and these include the greatest number of identical species. Plate after plate is made up of forms which are most familiar to every paleontologist of Europe and America. One plate is occupied by figures of *Pentamerus galeatus* Dalm., *Atrypa desquamata* Sowb. (hardly distinct from *A. reticularis*) and *A. aspera* Schloth; the next plate mostly by *Spirifer Verneuili* Murch. (*S. disjunctus* Dav.); one overgrown by *Aulopora tubæformis* Goldf., another carrying *Crania obsoleta*, *Spirorbis omphalodes* and *Cornulites epithonia*, all described by Goldfuss from European specimens. Of the eight Orthides two only are exclusively Chinese. Among the Carboniferous brachiopods nearly all are identical with ours, the first plate which represents them being composed of *Retzia compressa* Meek, *Athyris globularis* Phill., *Spirifer lineatus* and *S. glaber* Martin.

On the next plate are represented *Streptorynchus crenistria*, world-wide in its distribution, our *Meekella striato-costata* Cox and *Terebratula hastata* Sowb. On the next with *Orthis Pecosii* Marcou are several figures of *Syntrielasma hemiplicata* Hall (*Spirifer hemiplicata* Hall, Stansbury's Salt Lake Report) exactly like our peculiar fossil.

An interesting review of the Devonian fauna is given by Kayser on page 98, from which it appears that of the twenty-eight described species only six are peculiar to China and of these only two, *Nucleospira Takwenensis* and *Orthis Richthofeni*, are distinctly different from any found elsewhere; the other four Chinese species having representatives in Europe or America, to which they are so closely allied that they may perhaps be only varieties.

On page 199, Kayser begins a still more interesting and suggestive review of the Carboniferous fauna, in which he enumerates 55 species, only ten of which are considered as new. But two new generic names appear in the list, one a coral, *Richthofenia*, first obtained from India, and the other a fish tooth, *Leptodus*, allied to—if indeed it is generically distinct from—*Deltodus cingulatus* N. and W. from Illinois.

Figures of Cambrian trilobites occupy two plates. These are wonderfully like the illustrations of the Primordial fauna given by Angelin from Sweden, Hall from New York and Walcott from Nevada, and evidently represent essentially the same group of organisms.

It has been urged that our representation of the Primordial fauna is too fragmentary to permit generalization in regard to its zoological character and affinities. But we have now gathered from more than twenty localities—some of these antipodal—a group of Primordial fossils which exceed six hundred in number; and of these more than one half are trilobites. The repeated recurrence of this group to the exclusion of other fossils in rocks which hold a fixed relative position in so many and widely separated localities affords demonstrative evidence that they typify the animal life of the age.

The list of genera and species given by Herr Dames has not quite as familiar a look as his plates, for to some well-known forms he has assigned new generic names; for example, to the trilobite best represented in the collections, which has the pygidium armed with strong spines, the penultimate pair being much the longest, he has given the name of *Dorypyge*. Into this genus he brings two trilobites from Utah described by Hall, in King's Fortieth Parallel Report, under the names of *Dikellocephalus quadriceps* and *D.? gothicus*. Similar trilobites, and probably belonging to the same genus, have been found by Walcott in Nevada and by the writer of this notice in Southern Utah at the same geological horizon. *Lyostracus* and *Anamocare* are names which will be less familiar to paleontologists than *Conocephalites* and *Agnostus* with which they are associated in Herr Dames's notes; but they are Angelin's names, given to Primordial trilobites which had before been included in *Crepicephalus* and *Conocephalites*. In *Anamocare* are included such species as *Conocephalites hamulus*, *C. Wisconsensis* and *C. Patersoni* of Hall.

To the fossil plants collected by Baron Richthofen are given 60 of the 269 pages of text and 25 of the 54 plates which compose the volume. As is usual in collections of fossil plants, much of the material representing large organisms of complicated structure is fragmentary, and to the fragments more significance is sometimes given than is warranted; still a sufficient number of well-marked and characteristic species are represented to make the general conclusions reached by Herr Schenk safe and of great interest. The fossil plants were collected from twelve localities and represent three geological horizons, the Carboniferous, Jurassic and Tertiary.

The Carboniferous species amount to about 40, of which perhaps 30 are sufficiently complete to be satisfactorily identified. Of these, nearly all the most distinctly marked are identical with European Carboniferous species or are very closely allied to them; such as *Calamites gigas*, *Annularia longifolia*, *Neuropteris flexuosa*, *Cyatheites arborescens*, *Sphenophyllum truncatum* (a variety of *S. emarginatum*), *Stigmaria ficoides*, *Cordaites principalis*, etc. Among the new species described is a large *Annularia* with very numerous and contiguous leaves, to which Herr Schenk has given the name of *A. maxima*; but which Zeiller suggests is not distinct from large forms of *A. stellata* Schloth. (= *A. longifolia* Brgt.) (*Annales des Mines*, Oct., 1882). Three species of *Archæopteris* are noticed, for which Geinitz's anticipated name *Palæopteris* is retained. Of these one, "*P. lanceolata* Schenk," figured on page 218, is probably a group of the abnormal folioles which constitute the summit of the pinnæ in some species of *Neuropteris*, as *N. cordata* Brgt. and *N. Rogersorum* Kimball.

The most striking of all the coal plants is a large ovoid leaf like that of a Magnolia, which Schenk has called *Megalopteris*. The name is preoccupied, having been given by Dawson in 1871 to some large Neuropterids with forking pinnæ, and further illustrated by Andrews in the Paleontology of Ohio, Vol. II. The plant is of special interest, and it is to be hoped that Herr Schenk will choose a new name for it, and that its structure and relations will be more fully made out by further collections.

The Mesozoic plants described are from seven localities in China and one in Japan, and they pertain to twenty-nine species, of which nearly all the most distinctly marked have been before obtained from India, the Amoor, Turkestan or Europe. Conspicuous among them are the omnipresent *Asplenium Whitbyense* (*Pecopteris Whitbyensis*), *A. argutulum* and *Podozamites lanceolatus*. They are considered as indicating a Jurassic age for all of the strata which contain them: of this, however, there may be a question, since forms identical with, or allied to, many of those described have been obtained from the Rhætic beds in other parts of the world. If, however, the Rhætic is to be considered as the base of the Jurassic system, and not Triassic—a view which is gaining ground in Europe—there can be no objection made to this conclusion.

The only thing which requires notice among the Jurassic plants is the large frond figured on Plate 51, Fig. 1, regarded by Herr Schenk as *Clathropteris*, but which wants the characteristic retangular nervation of the genus. The principal nerves are dichotomously forked, and it seems more likely that the fragment represents a new species of *Dictyophyllum.*

A notice of this elegant volume would be incomplete without reference to the exhaustive study by Schwager of the Carboniferous Foraminifera of China and Japan. These form "Fusulina limestones" in the Old World as well as in the New, composing in China, as in Kansas, Utah and California, the greater part of strata many feet in thickness. As in America these singular bodies are sometimes fusiform and sometimes spheroidal. For the former the old name of *Fusulina* is retained, and the spheroidal ones are made to represent a new genus, for which the name *Schwagerina* is chosen by Möller. These are illustrated by four plates and nearly a hundred figures, many of which are highly magnified sections, which will be useful for comparisons with *Nummulites* and with the supposed organic structure of *Eozoön.* J. S. NEWBERRY.

6. *Note on Professor R. D. Irving's paper on the Paramorphic origin of the hornblende of the crystalline rocks of the Northwestern States;* by M. E. WADSWORTH. (Communicated.)—Since Professor Irving's paper in the July number of this Journal appears to imply that no one except Dr. Wichmann has ever studied microscopically the rocks of the Marquette district, and no one except himself advocated the secondary origin of the hornblende in the "greenstones," or the eruptive origin of the diabases, a brief statement of the facts is necessary.

Chas. E. Wright published, in 1873, descriptions of the microscopic characters of a suite of Marquette rocks;* while a more extended study by myself was published in 1880.† In my work the secondary origin of the hornblende in the "greenstones" was distinctly advocated and the rocks classified accordingly.‡ The olivine-bearing character of part of the diabases was then first pointed out, and the supposed serpentine of Presque Isle was first shown to be a peridotite of the lherzolite variety; also the trachytic and rhyolitic nature of the Keweenaw felsites, since advocated by Irving. The eruptive origin of the diabases as held by Messrs. Foster and Whitney was also clearly proved by figures and sections.

Since we agree on these points and since a copy of my published paper was sent him at his special request, it is difficult to understand why Professor Irving should ignore my work, and publish the same views as original with himself.

Cambridge, Mass., July 10, 1883.

7. *Earthquakes of September* 7, 1882, *in Central America.*—A letter from Mr. A. Ernst, of Caraccas, in the July number of this Journal calls attention to an error in my last Notice of American Earthquakes (p. 357, May, 1883). He states that this earthquake was *not* felt at Caraccas as stated by me. The state-

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* Geol. Mich., 1873, ii, 215–231. † Bull. Mus. Comp. Zool., 1880, vii, 28–76.
‡ l. c. pp. 37, 39, 42, 46, 70, etc.

ment was made on the authority of an article in the Atlantic Monthly (March, 1883, p. 397), describing the personal experience of the writer (H. D. Warner), in an earthquake occurring at 2.20 A. M., September 7. The year was not given, but it was a natural inference that it referred to the shock felt in adjacent districts at 3.20 A. M., September 7, 1882. On looking again at the article I see that it *might* refer to some previous year, and I presume that is the fact. Probably Mr. Ernst could tell what year it was.

The correction needed to the notice on page 357 will then be to *erase all reference to Ecuador and Venezuela.*

May I take this opportunity to say also that, as my notes are necessarily based largely upon newspaper notices, which are not always reliable, I shall be glad to receive corrections to my published lists from any persons who may have better information; or, better still, I shall be glad to receive and acknowledge any reliable notes sent me in advance. My permanent address is as below. C. G. ROCKWOOD, Jr., Princeton, N. J., U. S. A.

July 13, 1883.

8. *Brief notices of some recently described minerals.*—EMPHOLITE: Described by Igelström as occurring in radiated and fibrous aggregates of very minute but well-formed crystals. The crystals have been examined by Bertrand and shown to be orthorhombic, and near diaspore in form, having a prismatic angle of 128° to 130°; the planes 110 (*I*), 120 (*i*-$\breve{2}$), 130 (*i*-$\breve{3}$), 010 (*i*-$\breve{\imath}$) cleavage, perfect parallel 010. The optic axes lie in the brachydiagonal section, the acute positive bisectrix parallel to the shorter lateral axis, $2V = 85°$. Hardness, about 6. Color, white, becoming yellowish on exposure to the air from oxidation of the iron. Infusible, and does not decrepitate, and very slightly, if at all, attacked by acids. Two analyses gave—

	SiO_2	Al_2O_3	MgO, CaO, FeO	H_2O
1.	51·70	31·52	4·60	12·18 = 100
2.	47·86	33·60	5·70	12·84 = 100

These results the author corrects for the 16 p. c. gangue intermixed, and obtains

	SiO_2	Al_2O_3	MgO, CaO, FeO	H_2O
1.	52·3	30·5	3·4	13·8
2.	48·8	33·3	3·3	14·6

The mineral is essentially a hydrous silicate of alumina, but owing to the impurity of the material analyzed the exact composition must be regarded as very doubtful. It occurs mixed with cyanite and in cavities in schistose damourite and in quartz; from the Hörrsjöberg, Wermland, Sweden.—*Bull. Soc. Min.*, vi, 40, 1883.

IGELSTRÖMITE, SILFBERGITE and MANGANHEDENBERGITE are manganese minerals, described by Mats Weibull, from Vester-Silfberg, in the Norrbärke parish, Sweden. Igelströmite occurs in grayish-black crystalline masses of irregular texture, sometimes almost homogeneous, and again mixed with a carbonate of lime and manganese, and magnetite. Cleavage in two directions, making an angle of 131° with each other, and a third, indistinct, at right angles. Specific gravity, 4·17. Luster, vitreous to greasy. Translucent with yellowish color. An analysis gave—

SiO_2	FeO	MnO	Mgo	$CaCO_3$
29·94	46·88	18·83	3·01	1·14=99·80

This corresponds to the chrysolite group, the mineral being intermediate between fayalite (Fe_2SiO_4) and knebelite $FeMnSiO_4$, it having the ratio of Fe : Mn=2 : 1. The same mineral has been previously described by Igelström under the name of knebelite, of which it is properly a variety. (The name igelströmite has been used by Heddle; see 3d Append. Syst. Min., p. 99.)

Silfbergite is a honey-yellow mineral resembling actinolite. It occurs in bladed crystals and crystalline aggregates showing two prismatic cleavages like hornblende, also in nearly compact masses. Luster, vitreous. Hardness, 5·5, and specific gravity 3·446. Translucent, slightly dichroic. An analysis gave—

	SiO_2	FeO	MnO	MgO	CaO	ign
(⅓)	48·83	30·49	8·34	8·39	1·74	0·44=98·23

This brings it near anthophyllite, from which it seems to differ only in its percentage of manganese. Silfbergite occurs with the above described igelströmite.

Manganhedenbergite is a grayish-green pyroxenic mineral, very near hedenbergite, but characterized by its containing a few per cent of manganese. Hardness 5, and specific gravity 3·55. An analysis gave—

SiO_2	FeO	MnO	CaO	MgO	$\underbrace{Na_2O, K_2O}$
48·29	24·01	6·47	17·69	2·83	0·22=99·51

Together with the above minerals three other manganesian species are described, a manganese garnet, a carbonate of lime and manganese, and wad.—*Geol. För. Förh.*, vi, 499, 1883.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *A visit to Ceylon*, by ERNST HÆCKEL. Translated by Clara Bell. 338 pp. 8vo. Boston, 1883. (S. E. Cassino & Co.)—Hæckel, in this book of travels, is not the investigating and theorizing biologist which his name brings before the mind, but the man of general culture—science not excluded—finding delight in nature and in the people about him, as new scenes opened to view in his journeyings. His descriptions—which embrace all kinds of subjects that suggest themselves to an observing traveler, and much that only one with an eye quickened by science would see—are always excellent and often amusing. The tropi-

cal forests and landscapes, and the coral and other scenery of the waters, gave him great pleasure, and the reader derives much of that pleasure from the perusal of his glowing but never overdrawn sentences. The volume, in its English dress, is an attractive one in all respects, and not the least so for the portraiture it gives through its pages of the eminent biologist.

2. *Handbuch der Klimatologie;* von Dr. JULIUS HANN, Direktor der met. Zentralanstalt, und Professor an der Univ. in Wien, etc. 764 pp. 8vo. Stuttgart, 1883: (J. Engelhorn).—This volume is one of the series constituting the Bibliothek Geographischer Handbücher, edited by Professor Dr. F. Ratzel, and several eminent collaborators. It is a valuable work, giving in compact form a large amount of information in regard to the climates of the different parts of the earth. It opens with an introduction devoted to a statement of the various climatic elements; that is, the temperature and its changes, the humidity of the atmosphere, and the precipitation of rain, snow or dew, the clouds and fog, winds, atmospheric pressure, evaporation, and composition of the air. The work proper is divided into two parts, the first containing a discussion of general climatology considered under the two heads of *the solar climate*, or the climate of the earth as determined simply by the amount of solar radiation and the character of the earth's atmosphere. The second head is the *physical climate*, or the actual terrestrial climate as influenced by the modifying causes of the varied character of the earth's surface, of land and water with the accompanying currents of air and sea, and the unequal altitudes of different parts of the earth above the sea surface. Under the second head the characteristics of land and sea climates on the one hand and of the mountain climate on the other, are discussed at length. This portion of the work, comprising about one-sixth of the whole, contains much useful matter; the causes of the varying temperature over land and sea, the influence of the continents on the moisture of the atmosphere and the amount of precipitation, the influence of the land on the wind in the different seasons, the influence of ocean currents on the climate, of mountain chains upon the precipitation of rain or snow are some of the topics which are discussed. The second part of the volume is devoted to special climatology or the description of the climate of the different parts of the earth, first for tropical regions, then for the temperate zones, and finally for polar regions. The considerable space, upwards of 500 pages, given to this portion of the subject allows the author to present the climatic peculiarities of the various regions in considerable detail, and whether the reader is interested in the gold coast of Africa, in Alaska, or the Sandwich Islands, he will find here a good summary of the results of past observations.

3. *Manual of Taxidermy: a complete guide in collecting and preserving Birds and Mammals;* by J. C. MAYNARD. 112 pp. 12mo, with illustrations. Boston, 1883. (S. E. Cassino & Co.)—

The author gives his facts and advice briefly as well as clearly, and hence brings the whole within a comparatively few pages. The work takes up, in order, the subjects of collecting by trapping, shooting and in other ways; skinning and preserving; making skins—under which head varying directions are given according to the tribe; labelling, mounting, etc. Birds are first considered, and after these, Mammals; and much more briefly, Reptiles, Batrachians and Fishes.

4. *Dredging in* 1880 *by the U. S. Coast Survey Steamer Blake: Report on the Ophiuroidea;* by THEODORE LYMAN. Vol. x, No. 6, of the Bulletin of the Museum of Comparative Zoology, pp. 227–286, with 8 plates. Cambridge, May, 1883.

5. *Royal Society.*—Professor Huxley has been elected President of the Royal Society, in place of Mr. Spottiswoode, deceased.

6. *Professorship of Chemistry in the University of Virginia.*—A circular letter from Dr. James F. Harrison, Chairman of the Faculty of the University, states that an election to fill the Chair of Chemistry, including General and Industrial Chemistry, made vacant by the resignation of the present incumbent, will be held by the Board of Visitors of the University of Virginia, on the 11th of September, 1883. The salary of the Professor will be $3,000, payable in three equal parts, on the 15th of October, January and April, respectively, with a commodious house, recently built and provided with modern conveniences, rent free. Applications, with testimonials, must be addressed to "The Rector and Visitors," P. O. University of Virginia, Albemarle County, Virginia.

7. *American Association.*—The thirty-second meeting of the American Association, to be held at Minneapolis, will open on the 15th of August, under the presidency of Professor C. A. Young, of Princeton. A list of the officers of the meeting is given in volume xxiv of this Journal on page 304. The rooms of the Association will be at the State University. Professor N. H. Winchell is the Secretary of the Local Committee, and from him the circular of this committee, giving hotel charges, a list of the railroads (twenty or more in number) which will give reduced rates, and other information of value to the members, can be obtained. An illustrated guide to the city of Minneapolis will be distributed to the members.

Express packages containing apparatus, specimens, maps, books and other articles designed for use in the meetings of the Association, will be forwarded by the American Express Company and delivered free of charge at the State University. They should be addressed to the *care of Professor J. A. Dodge*, to whom all correspondence relating to the same should be sent.

All matters relating to membership, the presentation of papers, payment of assessments, etc., and business to come before the Association will be attended to by the *Permanent Secretary* at *Salem, Mass., up to August 8th;* and after this date, until the adjournment, at the Nicollet House, Minneapolis. Professor Putnam reports that by the generosity of General William Lilly, Nashville volume of the Proceedings has been reprinted, and the liberality of another member the St. Louis volume is being reprinted, while the several smaller gifts of a numbe members will be held until a sufficient sum is subscribed to rep volume 2. The copies of volume 24 are now nearly exhau and further subscriptions to the reprinting fund are greatly sired. Copies of 2, 24 and 27 will be received in exchange other volumes, or for assessments at the rate of a dollar a volu Members can complete their sets of Proceedings at the rat one dollar a volume, if ten or more volumes are ordered at time; under ten volumes the price is $1.50 a copy. The qu memoir on Fossil Butterflies by S. H. Scudder, printed by donation of Mrs. Elizabeth Thompson, is furnished by the Asso tion at $2.00 a copy; the Transactions of American Geolog and Naturalists, 8vo, 1843, at $3.00 a copy.

Liberal arrangements are made for the Association at Mi apolis, and it is confidently expected that the meeting will b large one.

OBITUARY.

Sir EDWARD SABINE, K.C.B., died on the 25th of June, in 95th year. General Sabine's name is known in science m prominently through his two long series of observations and cussions connected with terrestrial magnetism and the pendul Both series were begun over sixty years since, after voyages the Arctic with Sir John Ross and Sir Edward Parry, and former was continued until 1872, in which year appeared a pa giving a magnetical survey of the North Polar regions, to se as a companion to the survey of the South Polar region which already appeared. General Sabine discovered, simultaneou with Wolf and Gautier, the connection between unusual magn disturbances and large declination, and the year or time of m mum sun spots. His election to the Royal Society took place 1818, when in his 30th year. In 1821, he received from it Copley medal; in 1849, the Royal medal. In 1861 he was elec President of the Royal Society, which position he held for years. In 1869 he was made K.C.B. The Colonial Obse tories owed their existence to the efforts of General Sabine, a were for many years under his control.—*From a biograph sketch in Nature of July 5th.*

WILLIAM SPOTTISWOODE, President of the Royal Society, ment as a mathematician and physicist, and a scholar also oriental and other languages, died on the 27th of June. He buried in Westminster Abbey. Mr. Spottiswoode was born London, Jan. 11, 1825, but was of Scottish family. His earl [illegible] mathematical work, "Meditationes Analyticæ," appeared w he was twenty two years old. After 1870 his attention divided between physics and mathematics. A complete list of his papers is contained in Nature of April 26.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XIX.—*On the Existence in both Hemispheres of a Dry Zone, and its cause;* by ARNOLD GUYOT.

(Read before the National Academy of Science, Nov., 1882.)

OF the two elements of climate indispensable to vegetable and animal life, temperature and moisture, it is well known that the distribution of the latter is subject to the greatest irregularities. While the average annual temperature in each place varies but a few degrees, from year to year, the quantity of rain at the same place may differ from one year to the other by one quarter or even one half. In tropical countries where the temperature is almost constant, the quantity of aqueous precipitation shows the greatest extremes; for here we find both the rainless districts as well as the most abundantly watered regions. These irregularities render it all the more desirable that some general law should be found which may aid us in putting order in this apparent disorder, and in understanding the causes of these extraordinary variations.

One of the general laws already recognized is that the quantity of rain, on the whole, decreases with the temperature, that is, from the equatorial belt toward the poles. This was to be expected since the warmer the air the greater is its capacity for holding vapor. If the average quantity of rain within the tropics is from 90 to 100 inches, it is only 60 on the 30th parallel, 40 on the 40th parallel, 20 on the 50th parallel, 10 and less in the polar regions. This general law, however. experiences a remarkable interruption a little beyond either tropic, where the quantity of rain is suddenly reduced to a minimum, while on the equatorial side of that belt we find the abundant summer rains of the tropical climate, and on the polar side the copious winter rains of the warm temperate regions. The result of this scarcity of rain, in these particular latitudes, is apparent in the existence of a series of dry lands which, nearly all, are among those enumerated in the paper of Professor Loomis, just read, as having a fall of rain below 10 inches. (See this Journal, Jan., 1883.) I have shown elsewhere —in my Physical Geography, page 90—that these dry, parched tracts form two belts around the globe; two *dry Zones*, on both sides of the tropics, containing most of the so-called deserts of the world, in which the regime of the rains, as well as their quantity, differs from that of the neighboring climatic regions. As this fact, however, does not seem to have been fully recognized, perhaps for the want of a sufficient development, I beg leave to again call the attention of the Academy to it, and to indicate in a few words, what I believe to be the cause to which this remarkable phenomenon may be referred.

The Northern zone of dry lands extends in width from about 24° to 32° N. lat. (See Loomis's map of rains, in this Journal, vol. xxv, January, 1883). In the New World it begins at the west with the peninsula of Lower California; thence passing through Arizona, New Mexico and Western Texas. In all these lands, extending for nearly a thousand miles from west to east, the annual fall of rain remains below ten inches and goes down to two and three inches, while in some years the rain fails entirely. Farther east, in the same latitudes, local causes, to be mentioned hereafter, give abundant rains to the valley of the Mississippi and Florida.

In the Old World the dry zone occupies the very center of the Great Sahara, where the absence of rain is nearly complete on a length of 3,200 miles, and is considerably increased in width. Thence it crosses the central part of Arabia on a line of 1300 miles, passes through the dry plateau of Eastern Persia and Belochistan, and reaches, beyond the Indus, the desert of Thurr, after a course of another one thousand miles, making together a tract of 5,500 miles of dry lands.

Farther east, as in the New World, local causes bring, in the same latitudes, abundant rains which mask the influence of the general cause of dryness, as it will presently be shown.

In the Southern hemisphere the dry zone is strongly marked on the west slope of the Andes. It is well known that all the coast of Peru from Punta Parina 6° S. L. to N. Chili 30° S. L. is a rainless region; but it would be a mistake to believe that the atmosphere is wanting in moisture in all that extensive district. All along the Peruvian coast, though owing to special circumstances no condensation takes place near the seashore sufficient to produce rain, the thick winter fog, called the

Garua, furnishes moisture enough to render all hills and pastures green with verdure. In the latitude of the dry zone, however, from 20° to 30° S. lat., in the desert of Atacama, about the tropic of Capricorn the atmosphere is not only rainless but perfectly dry. Even on the coast of Chili, Copiapo, 27° S. lat., receives only 0·32 inch of rain, La Serena in Chili 0·5 inches. In the same lat. on the east of the Andes the plains of the Pampas are subject to great droughts. The *gran-seco* (great drought) of nearly three years duration, which took place in this century, is still remembered as having cost the life of millions of cattle which came to die along the few river courses which had retained some water; leaving their bones accumulated just as the fossil bones of the Quaternary mammals are now found in the Pampas.

In the same latitudes in Africa the Kalahari Desert extends over the whole western half of the continent and beyond. In Australia the dry zone passes from west to east through the very center of the continent between the summer rains of the tropics and the winter rains of the more southern latitudes, with the exception of a narrow strip of the eastern coast which is watered by the winds from the sea; condensing rather scanty and irregular rains on the Blue Mountains and the Australian Alps.

The existence in each hemisphere of a dry zone in the subtropical latitudes may thus be considered as a fact for which a cause must be found. On the other hand it is true that both in the northern and southern hemispheres notable interruptions are found, and it is to be remarked that they are all on the eastern side of the continents. In America from eastern Texas along the gulf to Florida the latitude of the dry zone has an abundance of rains. In the Old World from the Sind desert near the Indus through both Indian peninsulas and Southern China, the lands in the latitudes of the dry zone are still more plentifully watered. In the southern hemisphere the eastern half of South America and South Africa and the eastern coast of Australia, though moderately supplied, are not wanting in rain, especially near the sea. This apparent anomaly also has to be accounted for.

As among the causes which govern the aqueous precipitation the course of the winds occupies the first place, I am inclined to think that a close consideration of the normal movement of the atmosphere in the latitudes of the dry zones will account for the above mentioned facts.

These winds belong to two classes. In the first are those which form a part of the general circulation of the atmosphere, resulting from the spherical form of the globe. In the other are winds mainly due to the relative distribution of continents

and oceans and participating therefore in the nature of the mo
soons. To the first, I believe, the scarcity of rain in the d
zones must be attributed; to the others the abundance
rain in the well-watered portions of the same zones, which int
rupt the continuity of the dry lands.

The operation of these causes is as follows:

It is generally accepted that the primary circulation of t
atmosphere, arising from the difference of temperature betwe
the tropical and the temperate and cold regions of the glo
has its origin in the broad ascending current which, under t
influence of a powerful equatorial sun, starts from the belt
calms. This current carries incessantly into the upper regio
of the atmosphere vast masses of air which overflow on bo
sides toward the temperate zones, causing an accumulation
air about the 30th degree of latitude, enhanced still more
the decrease of the areas poleward. This accumulation
shown by the existence of a belt of high barometric pressur
which there reaches the maximum known on the surface of t
globe. From this belt of high pressure the air, as usual, divide
flowing downward both ways. A portion of the current retur
toward the tropics and becomes a part—perhaps the main pa
—of the trade winds and reascends in the belt of calms, ma
ing a complete circuit, as well shown by our colleague, Pr
Ferrel. The second flows poleward and becomes the ant
trades and the southwesterly winds characteristic of the te
perate zones. The part, however, which each of these bodi
of winds plays in the condensation of rains is very unlike; t
first brings drought; the second is the principal source of t
rains of the temperate latitudes.

It may, at first sight, appear singular that these two branch
of the same aerial current should have so different an effect
aqueous condensation. A moment's reflection, however, wi
make the reason clear and dispel our surprise. Both are d
scending currents; as such they will be dry winds; for a d
scending wind forced into a region of greater pressure will co
tract; in so doing the latent heat which it has absorbed i
ascending, reappears and increases its temperature and capacit
for moisture, thus rendering condensation more difficult,
not impossible. But when traveling farther, circumstanc
change. The equatorial branches blowing toward warm
regions, with no chance to meet a cooler atmosphere, lose a
regular causes for condensation, and the countries they pa
over remain dry and parched. The polar branches, howeve
traveling toward cooler latitudes, their relative humidity gra
ually increases at every step and rain will soon ensue.

Moreover both currents gradually diverging toward opp
site quarters, before they reach the ground, leave betwee

them a neutral zone, where the motions of the air will be few and irregular, forming the sub-tropical calms, or horse latitudes, where ascending currents or cool winds, which might bring rains, are almost impossible. Wherever such conditions are found a *dry zone* is sure to appear. Thus the existence of dry zones in the latitudes above mentioned is not an accidental phenomenon, only due to local causes, but it depends upon the laws of the general circulation of the winds.

If it be so it is evident that modifications in that system will affect the position of the dry zones. The southern zone, for instance, is somewhat nearer the tropic for the reason that the middle, or central line, of the belt of calms is on the north of the equator, removing the whole system of trades and anti-trades toward the north, an effect due, no doubt, to the predominant extent of the land masses in the N. Hemisphere.

Though this cause of dryness in these particular latitudes may be considered as by far the most efficient, secondary causes are not wanting which intensify locally the phenomenon, or give it a greater extension. It is to be noted that most of these desert regions are plateaux, with the exception of the outskirts of the Sahara, the Indian desert (of Thurr) and central Australia. Plateaux always increase the dryness of the atmosphere above them for several reasons. Those usually given, such as their distance from the sea, the decrease in the supply of moisture owing to the condensing action of their border mountains, are no doubt efficient causes; but they are by no means the only ones. Perhaps one of the most active is the elevation above the sea level, into the continental atmosphere, of such large portions of the earth crust. Their surface exposed to the increased power of the rays of the sun, in a usually clear sky, with diminished density of the air, becomes strongly heated, keeping the air on the plateau warmer than the air at *the same level* above the neighboring sea or lowlands. It is evident that in such circumstances the moisture brought by cooler winds, blowing from the sea into the warmer atmosphere of the plateaux, will have no chance to condense, except on the mountain chains which may happen to break the monotony of their surface, as we see it in the vast plateau region between the Sierra Nevada and the Rocky Mountains.

The great scarcity, or even complete absence, of vegetation, the bare, or sandy soil, usually characteristic of the desert, favors in these subtropical latitudes an accumulation of heat which adds another element of dryness, and which tends to increase the breadth of the dry zone.

The rains, often quite abundant, which fall in the portions of the subtropical zones situated on the east side of the great land masses and which have been indicated above as notable interruptions of the dry zones, are due, not to the general currents resulting from the spherical form of the globe, but to the powerful influence of the relative situation of the great land masses and water areas, producing strong monsoon winds. These monsoons are nowhere more marked than on the southeast portion of the huge mass of the Asiatic continent, where they overcome the general wind circulation, extending even far beyond the dry zone, as Woeikoff has shown, at least as far as Japan. In North America, owing to the triangular form of the continent, its largest portion is in the north. This extensive land area being overheated during the summer, the southwest returning trades are deflected during the warm season toward that region of greater heat and low pressure, and are changed into the south winds which prevail at this time in the valley of the Mississippi and bring from the warm waters of the Gulf and the southeast Atlantic the abundant vapors and the summer showers which greatly increase the average amount of rain in that region.

Thus is explained the anomaly observed in the eastern third of the continent of North America, namely that contrary to the usual law, the amount of rain on the same parallel instead of diminishing from the coast toward the interior, rather increases to a distance of a thousand miles from the sea-shore.

Only a few words more on the name of the different zones of rains mentioned in this paper. It has been usual, for a considerable time, to call the region of the winter rains the *subtropical* rain-zone. As these rains very often extend as far as the 40th parallel, as in California and elsewhere, while they do not reach the region of the alternate tropical summer rains, the name *subtropical* seems rather to be a misnomer; the dry zones, as above defined, are really the true sub-tropical zones. From the equator northward we would thus have 1st, the equatorial zone of daily rains; 2d, the zone of alternate tropical rains and dry seasons; 3d, the subtropical or dry zone; 4th, the zone of winter rains and dry summers proper to the warm temperate regions; 5th, the zone with prevailing equinoctial rains, or the temperate regions; 6th, the zone of prevailing summer rains and cloudiness, or the cold temperate region. Though these zones somewhat vary their position in latitude, according to local circumstances, they can always be recognized, and they agree with the average distribution of temperature and with the circulation of the winds.

ART. XX.—*On the Relations of Temperature to Glaciation;* by GEORGE F. BECKER.

THE theory of a Glacial period has always presented great difficulties. Agassiz acknowledged their existence from the first, and stated that his hypothesis seemed to conflict with the apparently well-established theory that the temperature of the earth is gradually and progressively sinking. His investigations led him to the belief that the decline in temperature was not uniform, but marked by the occurrence of repeated minima. Dr. Frankland, Baron Sartorius von Waltershausen, Mr. Clarence King and others have pointed out that to account for the enormous accumulation of ice during the Pleistocene, it seemed necessary to assume an evaporation much more active than that of the present day, and which could scarcely be accounted for except on the theory of the prevalence of higher temperatures. Prof. Whitney has lately advocated a similar view, while the general opinion among geologists has been that to account for the accumulation of ice in question, a lower temperature was requisite. This view has recently been sustained by Mr. G. K. Gilbert in opposition to the arguments presented by Prof. Whitney. Since it is evident that the question cannot be settled without further discussion, it is probably desirable to consider it from as many points of view as possible. Of these one will be here presented.

If the sun and earth are members of a system now undergoing a uniform loss of heat, it appears certain that the formation of glaciers must be limited to a distinct period in the earth's history. So long as the temperature nowhere sank, even during the winter months, as low as the freezing point, it is manifest that no glaciers could form. A moment's reflection will also show that a sufficient diminution of temperature on the earth's surface would preclude the formation of glacial ice, for the rate of evaporation diminishes more rapidly than the temperature. The entire quantity of water existing in saturated aqueous vapor at a temperature of minus 20° C. is a little less than would be precipitated from aqueous vapor of a temperature of plus 20° by a reduction of only a single degree. Since the precipitation must equal the evaporation, the snow-fall in the later stages of the history of the earth must therefore become infinitesimal. The present time lies between the extreme periods indicated, and it is known by observation that glaciers are in process of formation. It is certain, therefore, that the development of glaciers on a cooling earth, must increase to a certain maximum and again decline.

The formation of glaciers must be confined, under otherwise equal conditions, to areas lying between certain isotherms. If a mountain is supposed to rise in a tropical country to an indefinite height, a temperature will prevail at its foot too high to allow of the formation of ice, and its peak may be supposed to stand at an elevation where the cold is too great to permit of any considerable precipitation. Dr. Tyndall has drawn attention to this fact and pointed out that, were the mountains high enough, there would be an upper as well as a lower snow line. If the earth is a cooling body, this snow belt must gradually be descending the hypothetical mountain suggested, and localities once too warm for the formation of névé must subsequently have been occupied by this incipient form of glacial ice, and again have grown too cold to permit of its formation. It appears, therefore, that not only the general development of the glacial system of the earth, but individual glaciers must, upon the current hypothesis concerning the cooling of the solar system, pass through a maximum.

The prime condition for the formation of a glacier is the accumulation in a basin of a quantity of wet snow, greater than will melt in the course of the season. The overflow of this basin takes place in the form of the glacial stream, which descends to a lower level until a temperature is reached at which the melting is so accelerated that the combined fusion and evaporation reëstablish the equilibrium between dissipation and solid precipitation. As observations show that both the névé and the glacier maintain substantially a constant temperature, that of melting ice, it is clear that both fluid and frozen water are essential constituents, since otherwise the temperature must vary. This fact is also readily explained, for the conversion of snow to ice, as is well-known, takes place by regelation which, under ordinary pressures, occurs only at the freezing point. It is true that the melting point of ice is lowered by pressure, and consequently it is conceivable that snow cooled below zero should accumulate at a great elevation in such masses, that the pressure upon its lower surface would be sufficient to produce fusion and union of the frozen particles of ice. In nature, however, this process can scarcely go on except in the immediate neighborhood of the zero point, for long before a column of snow cooled even a single degree below zero would be sufficient to produce this effect,* either the pressure would be relieved by avalanches or the light dry powder would be blown away by the tremendous gales which prevail at great elevations during the winter months. A portion of the snow thus removed from the higher parts of the snow belt will lodge at lower levels in névé basins, and the snow falling upon these basins from the clouds at temperatures considerably below the mean will also in part be swept away, and not contribute to the mass of névé. The actual precipitation on a névé field is, therefore, not an accurate gauge of the frozen water which it receives.

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* According to Sir W. Thomson's experiments, and Prof. Clausius's computations (Clausius' Mech. Wärmetheorie, i, 172), a pressure of one atmosphere depresses the melting point of ice 0·00733° C. It follows that a pressure of about a ton per square inch, or many thousand feet of snow, would be required to reduce the temperature of fusion 1°.

While both snow and rain, or melted ice, are essential constituents of the névé, it is evident that the formation of the glacial ice will not be most rapid at the extreme lower edge of the snow belt, for if the heat received is more than sufficient to raise the frozen water to the temperature at which regelation is most easy, the excess of heat will produce further liquefaction, and diminish the accumulation. This, therefore, will be most rapid, and the resulting glaciers largest neither at the upper nor the lower limit of the snow belt, and, other conditions remaining equal, it appears certain that the maximum accumulation will take place along a certain isothermal line, the temperature of which will not in general be very far removed from zero, though its precise temperature will depend upon local circumstances, such as the average humidity; the shelter from the direct rays of the sun, the distribution of moisture through the year, etc.

As has already been seen, the formation of glaciers is dependent upon topographical, as well as meteorological conditions. In order therefore to institute a comparison between the tendencies to glacier formation at two given periods of the earth's history, it is convenient to begin by assuming entire similarity between the topographical characters of the areas covered by the snow belts at the two epochs. Suppose the surface under consideration to be substantially an inclined plane of limited width but of an indefinite extension upward, this surface being merely roughened by depressions suitable for the accumulation of névé. Suppose also the proportion of sunshine and the strength of the winds to be the same at the two periods, then during the warmer epoch the maximum tendency to glacial formation will exist along a higher line, and during the colder period along a lower line upon the inclined plane. Both of these lines will be horizontal, and their temperatures will be nearly the same. The question to be decided is upon which of them will glaciers form most rapidly.

The relations of temperature to elevation are known to be involved in much obscurity, the degree of heat varying not merely with the pressure (Poisson's law), but being affected by the irregular ascent of warm currents from the earth, by the liberation of heat attending the precipitation of rain and snow, by topographical conditions, etc. The higher the temperature of any locality, however, the more rapidly the thermometer is found to fall as neighboring mountains are ascended. The rate of decrease of temperature with increasing altitude is greater in low latitudes than in high ones, and in one and the same locality is greater during summer than in winter, and greater during the warm hours of the day than before dawn.* This relation holds good even for very lofty elevations such as the summit of Pike's Peak, and there seems no reason to suppose that it does not extend over the snow belt. If so, the diminution of temperature with increasing altitude must have been at least a little greater in the snow belt of the warmer period than on that of the cooler period on the inclined plane supposed. Precipitation is mainly induced by the cooling of moist air as it rises along declivities, and, other things being equal, the precipitation will therefore be measured by the rate of diminution of temperature. It follows that the precipitation within a given distance of the line of maximum glacier formation of the warmer period, probably exceeded the precipitation within the same distance of the corresponding line in the cooler period.

The other conditions, however, would not be equal. Although the mean temperature of the air within the two belts might be identical, the heat of the direct sunshine would be more powerful during the warm period than at the later date, and consequently ice and snow exposed to the sunshine would melt more rapidly. Neither would the conditions at sea level be the same. Not merely would more heat be employed in evaporating water from the ocean during the warmer period, but the temperature of the ocean itself would be increased, especially near the surface, but also throughout the whole depth of shallow seas. As is well known, the rate of evaporation depends primarily upon the temperature of the surface of the water, and it follows immediately from the formulas which state the observed relations between temperature and tension of aqueous vapor, that the more intense the radiation from the sun, the greater will be the proportion of the heat employed in the evaporation of water. Appeal to observation shows that the operation of this law is not substantially obstructed by the action of any other of a contrary tendency. The heat received from the sun upon a square mile of the earth's surface, in latitude 45°, allowing for the absorption of the atmosphere, must be somewhat more than one half of the amount received upon an equal area at the equator, while the average rainfall of latitude 45° is only one-third of that at the equator. The rate of evaporation of still water in the two latitudes must differ considerably more than is thus indicated; for a large part of the moisture evaporated in the tropics rises many thousand feet above the surface and is carried poleward by the upper air-currents, while the surface of the sea in high latitudes owes a portion of its temperature to heat absorbed from the sun's rays near the equator. It is perhaps impossible in the present state of knowledge to make any exact comparison between the amounts of moisture reaching an elevation equal to that of the glacial line in the tropics and in higher latitudes, but enough is known to make it certain that the excess near the equator is very great, while the fact that, in spite of the transference of much of this vapor to the temperate zones, the snow line in the tropics reaches a lower isotherm than it does north or south of them, shows that the greater intensity of the sun heat above the snow line near the equator does not counterbalance the greater precipitation on the snow belt. Experience, therefore, amply justifies the statement that in the absence of meridional, oceanic and aërial currents, the effect of which will be considered later, the precipitation on the earlier and higher snow belt of the supposed inclined plane would exceed that upon the corresponding belt of a later and cooler era more than in proportion to the intensity of solar radiation.

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* See Loomis's Meteorology, p. 40; Report of Chief Signal Officer, 1876, p. 348; and Professional papers Signal Service, II, Isothermal Lines of the U. S.

The melting of ice at the glacial line, on the other hand, must be simply proportional to the heat received from the sun, since its temperature cannot be raised. Evaporation would be no greater during the warmer period, for supposing the air over the snow belts of the two periods to have the same temperature, that characteristic of the glacial isotherm, and that the saturation of the atmosphere were the same, the evaporation would of course remain unchanged. As has been shown, however, the rate of decrease of temperature, and the mean saturation, will probably be greater in the warmer period, and evaporation near the glacial isotherm will therefore be less.

If the inclined plane of limited width, considered in the foregoing paragraphs, also had a vertical limit, and if the sun has been gradually and uniformly cooling during the past, this slope can have had no glaciers until the lower snow line reached its upper edge. The maximum development of glaciers on this slope must have occurred when what has been called the glacial isotherm coincided with its upper edge, and since that time the magnitude of the glaciers upon the slope must have been constantly diminishing.*

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* It is of course to be understood that simple glaciers only are referred to, i. e. that a single glacial stream is supposed to drain only a single névé basin. In point of fact a large glacier frequently drains many névé basins at various altitudes. Such a glacier on the hypothetical slope might remain of constant magnitude during a long period of time, the increase of accumulation in lower névé basins compensating for the diminution in higher ones.

The conditions which exist in the mountainous regions of the earth's actual surface are of course widely different from those of the hypothetical declivity. The maximum development of glaciers cannot in fact have occurred when the glacial isotherm touched the top of the highest mountain peak; for though the meteorological conditions were eminently favorable, insufficient space was afforded for the accumulation of névé. As the isotherm descended the slope of the mountains, the topographical condition became more favorable, while the meteorological tendency to the formation of glaciers decreased. The actual maximum development of glaciers on a given group of mountains therefore depended upon the maximum value of a function of two variables standing in an inverse ratio to one another. Of these, that which expresses the suitability of the topography to the accumulation of névé is incapable of general discussion and unquestionably varies not only with the group of mountains but with different portions of the same group. It is even conceivable that there should have been such fluctuations introduced by the character of the topography as to bring about alternate retardation and acceleration in the growth of glaciers or an equal development at widely removed periods of time, but these must be regarded as extreme cases, and in general there must be in the history of every group of mountains a period of maximum glaciation. It is by no means to be inferred, however, that this period was simultaneous in different groups of mountains, still less so at remote portions of the globe.

Various influences tend to prevent the formation of glaciers, such as the heat of the atmosphere, the heat of direct sunshine, the heat accompanying warm rains, and the velocity of air currents. The importance of these various influences differs greatly, and is scarcely what, on first thought, might be supposed. The mean temperature of the atmosphere may vary within very wide limits consistently with the existence of ice. It is a well known fact that in deep gorges, even in very temperate climates, snow and ice when protected from the direct rays of the sun often last far into the summer or even throughout the year, and in spite, too, of an active circulation of air. Glacial ice even accumulates in artificial openings in localities far removed from any mountain glacier. The bottom of the Dannemora iron mine, for example, an immense open pit some 500 feet deep, is covered with ice to a depth of many feet throughout the year, although the sun sometimes shines upon it for a small portion of the day. Here, however, though the circulation of the air must be considerable, and is accelerated by the hoisting, blasting and other operations, incident to the working of the mine, the cold air probably accumulates to a greater extent than is possible in the deep gorges in the White Mountains, the Adirondacks and elsewhere, in which ice is nearly or quite permanent. Such occurrences make it clear that the direct heat of the sun is of the first importance in the dissipation of glacial ice or névé.

A certain amount of névé is not melted but evaporated. Indeed, according to Professor Whitney, the snow on the southern side of Mt. Shasta diminishes in volume almost exclusively from this cause, but a fixed amount of heat employed in dissipation by evaporation must produce a far smaller effect in the reduction of accumulated névé, than if it were employed in merely melting the snow or ice, for evaporation can go on rapidly only in dry air; and since the air is an extremely poor conductor, and has a low specific heat, most of the latent heat absorbed in volatilization of the water must be withdrawn from the underlying snow, and will thus counteract the tendency to melting. Indeed, since it is said to be possible to freeze water by evaporation and radiation from its own surface under favorable atmospheric conditions, at temperatures much above the freezing point, it is *à priori* probable that in a very dry atmosphere evaporation from a snow surface may be so rapid as to preclude melting, just as Professor Whitney has observed. Even when this is not the case, volatilization must retard melting. The melting effect of rain is no doubt very considerable, though as a certain quantity of water appears to be essential to névé, some rain is probably not unfavorable to the growth of glaciers. The melting effect of rain upon snow can readily be calculated if the temperatures of the two substances are known. If that of the rain is plus 5° and that of the snow minus 5°, the melting of a weight of snow equal to one sixteenth part of that of the rain, will reduce the latter to zero.

Still air can have very little effect upon the névé; the influence of winds is much greater, and is probably proportional to their velocity. Their effect, however, is almost wholly in one direction, for as long as any ice remains, warm currents of air will continue to dissipate it, and evaporation will proceed even below the freezing point. The chilling effect of cold winds, on the contrary, will reach but a short distance beneath the surface, for ice is a bad conductor, and however far below the freezing point the upper layer of the névé may be cooled, the underlying mass will be but very slightly affected. Of the influences tending to dissipate glaciers, none would seem to have been more vigorous at warmer stages of the earth's history, excepting the heat of the direct sunshine; and this, though more intense, was perhaps of shorter duration, for the mere fact of greater precipitation probably involved more cloudy weather or a shorter exposure of the ice to sunshine.

Hitherto it has been assumed that the warming of the sea, the evaporation and the precipitation, leading to the formation of a particular glacier take place under a sun heat of equal intensity, as would happen if there were no ocean currents and all these processes went on in the same latitude. Were this the case the lines of maximum glacier formation would first touch the earth as infinitesimal circles at the poles, and would thence move toward the equator remaining parallel to it. At any given period, maximum glaciation would be an event of the past throughout those portions of the globe lying north of the northern glacial isotherm and south of the southern one, while between them it would not have been reached. A different result must necessarily follow from the presence of open seas, for it is the most familiar fact in physical geography that heated waters from the tropics move poleward. Let it be supposed that at a given time in any given latitude distant from the equator there were no oceanic currents, and that the maximum of glaciation compatible with this condition of things had just been reached. If the barriers to the movements of the sea were then removed, water more highly heated than that characteristic of the latitude would flow in from the direction of the equator. An increase of evaporation and of precipitation would of course result, and the glacial isotherm would withdraw to a slightly higher altitude, but the sun's melting power would be unaffected and an immense increase would take place in the accumulation of névé. The action of aërial currents would be similar.

In the tropics, the conditions would be reversed. When the world is so cold that the glacial isotherm intersects the mountain slopes near the equator, the sun will not have its full effect upon the sea which constantly loses heated waters and receives cold currents. The glaciation at the equator will therefore be less than it would have been, had the surface waters been prevented by land barriers from seeking the poles. The glacial isotherm at the equator will be slightly lowered by the cooling of the ocean, but not in full proportion to the decrease of temperature at sea-level.

It follows that the rate of decrease of temperature with altitude will be more abrupt at a distance from the equator, during maximum glaciation, than it will be at that line when glacial formation there reaches its most rapid development, and since both evaporation at sea-level and the rate of decrease of temperature in the snow belt will be greatest in high latitudes, it is there that the extreme development of glaciers in the entire history of the earth, must take place. This highest development or absolute maximum of glaciation will occur where the influence of the warm oceanic currents is greatest, or where the temperature of the surface waters is most raised above the temperature which they would possess were the water stationary. The present position of such a line could probably be ascertained, but in the past and warmer stages of the earth's history, more intense radiation from the sun probably stimulated these currents to much greater activity, and a somewhat different distribution of land probably modified their courses. It is quite sufficient for the purposes of this paper to point out that the absolute maximum lines of glaciation cannot have lain in the tropics, and can scarcely be supposed to have been further removed from them than the arctic circles; in short, it is most reasonable to suppose that the greatest development of glaciers would occur in somewhere from 40° to 70° of latitude. It is scarcely necessary to point out that this inference accords extremely well with the geological records of an ice age, though not with the hypothesis of a polar ice cap.

If the generally received view, that the sun is a gradually cooling body, is correct,* it appears therefore nearly certain that the absolute maximum in the development of glaciers is past, and that the glacial period was not one of general cold, but one of higher mean temperature at sea level than the present. This is not equivalent to a denial that other causes than those discussed have had an influence, and perhaps a great influence upon glaciation. Indeed, it seems more probable that the formation of glaciers was affected by all contemporaneous changes, such as extraordinary upheavals and subsidences or periodic fluctuations in the eccentricity of the earth's orbit; but if the reasoning offered is correct, it is not necessary to resort to such events to account for the occurrence of a glacial epoch while many years may elapse before it can be conclusively proved whether the causes adduced were quantitatively more or less than sufficient to account for all the facts inexplicable except as results of glacial action.

Office U. S. Geological Survey,
San Francisco, May, 1883.

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* It appears to me, I confess, that substantiation of this hypothesis would be by no means superfluous.

ART. XXI.—*Analyses of two varieties of Lithiophilite* (*Manganese Triphilite*); by S. L. PENFIELD.

THE author has already published* analyses of both triphylite and lithiophilite from the various known localities, and he regards it of interest to add to these two additional analyses, one of lithiophilite from Tubbs Farms, Norway, Me., a new American locality, the other of a variety from Branchville, Conn. The specimens from Norway, Me., are associated with quartz, albite and tourmaline, and are broken fragments of considerable size, blackened on the exterior by oxidation, to which the mineral is peculiarly liable, but in the interior perfectly fresh and of a light salmon color. Specific gravity, 3·398.

Two varieties of lithiophilite from Branchville have already been described from independent but closely contiguous deposits. The first of a light salmon-pink color, containing from three to four per cent of iron protoxide, the second of a light clove-brown color, containing thirteen per cent of iron protoxide. A third variety, from an independent deposit, is of a very pale bluish tint, of a brilliant luster, clear and transparent. Specific gravity, 3·504.

The analyses of these varieties gave the following results:

	Norway, Me.	Branchville, Conn.
P_2O_5	44·40	44·93
FeO	8·60	16·36
MnO	35·98	28·58
CaO	·78	·05
Li_2O	8·50	8·59
Na_2O	14	·21
H_2O	1·19	·54
Gangue	·12	·13
	99·71	99·39

These analyses fully substantiate the formula of an orthophosphate, $Li(Mn, Fe)PO_4$, already made out for the species, giving the following ratios:

	P	:	Mn with Fe and Ca	:	Li with Na
Norway	1	:	1·02	:	0·91
Branchville	1	:	1·00	:	0·91

If part or all of the water be taken as basic the ratios are still closer.

These analyses, taken together with those of triphylite and lithiophilite already published, make an interesting series, showing the close relation and gradual transition between the two extremes of a lithia-iron phosphate containing very little manganese and a lithia-manganese phosphate containing but little iron.

My thanks are due to Professor George J. Brush for kindly providing me with material for carrying out these investigations.

Sheffield Scientific School, June 22, 1888.

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* This Journal, III, xiii, 426; III, xvii, 226.

ART. XXII.—*On the Intensity of Sound.—I. The Energy and Coefficient of Damping of a Tuning Fork;* by CHAS. K. WEAD.

No one doubts that a sounding body is distributing energy, and that a sound-wave is one mode of transmitting energy; but in most practical cases the quantity of energy involved is so small as to elude easy measurement, and the problem is sometimes complicated by physiological questions. Attention has been directed mainly, in acoustical measurements, to finding the velocity of sound in various media, and to determining the vibration-frequency of the notes of the scale and of sounding bodies; this last problem has been solved experimentally in more than a score of ways, from the simple rod-tonometer that was so serviceable in the hands of Chladni and Scheibler, up to the refinements of Mayer's electrical method, and Rayleigh's, Macleod and Clarke's, Poske's and Michelson's methods, and König's fork-clock. But from time to time a number of contributions have been made to this subject of energy. Our text-books, however, so seldom refer to them that one is surprised on looking up the matter to see how much has been done; yet in so disconnected a way that considerable connecting matter is needed. It is hoped that this paper may supply a part of what is needed. In saying that our books give us little on the subject, of course it is meant few quantitative results; we find easily enough the law of inverse distances given, with the pretended proof, by striking four bells twice as far from the ear as a single similar bell is; the use of resonance cases and sounding-boards is familiar, of speaking and hearing trumpets, of the deadening effect of a non-homogeneous atmosphere, etc.; all these and similar things are pleasantly told, with more detail than in any other book I have met with, in Radau's Wonders of Sound.

In recent years the question of Energy has come up in new ways, as in finding the Potential at any point due to a vibrating body, and in deducing the equations of harmonic motion as is done by Lord Rayleigh, from assuming that the sum of the kinetic and potential energy in the vibrating body is constant; but here as before we have no numerical data given to help toward clear thinking.

We may consider our subject under three heads:

A. Instruments to which a store of energy is imparted at once, and which are then left to themselves: e. g. tuning fork, bell, and all stringed instruments, except the violin family.

B. Instruments that store up little or no energy, and so need a constant supply from outside: e. g. all wind and reed instruments, singing flames, and the voice.

C. Physiological questions; the loudness of sound, etc.; something of this must be anticipated under A and B. I have found very little in print bearing on A, more will be cited under B, while much has been done on the physiological side.

I. THE TUNING FORK.

All bodies of class A are deformed at the outset by a blow or otherwise; the potential energy thus acquired may be measured with comparative ease in some cases as will be seen: let the calculation and measurement of this be the first problem.

In any elastic body, within the limits for which Hooke's law is true, the potential energy of the distorted body $=V=\frac{1}{2}Wa$, where $W=$ the force applied, and $a=$ the distance through which the point of application of the force moves. If $P=$ the force when $a=1$, then (1) $W=Pa$ and (2) $V=\frac{1}{2}Pa^2$. Since practically a fork is very nearly a representative of a bar fixed at one end and free at the other (3) $a=\frac{4Wl^3}{bd^3E}$, where $l=$ the length of the prong, $b=$ its breadth, $d=$ the thickness, and $E=$ Young's modulus; for steel $E=2{\cdot}14\times10^{12}$ in C. G. S. units (Everett), and for König's forks $b=1{\cdot}4$ cm. and $d=0{\cdot}65$ cm. approximately; therefore (4) $P=2{\cdot}056\times10^{11}\div l^3=$ the force in dynes, which applied at the end of the prong will bend it 1 cm.; and

$$(5)\quad V=\tfrac{1}{2}Pa^2=\frac{1{\cdot}03\times10^{11}a^2}{l^3}\text{ Ergs.}$$

It would be more convenient to express V as a function of N, the vibration-frequency, than of l; this may be done by Mercadier's formula;* here as before $d=$ the thickness in cm., $l=$ the length of the prong, which I take as the length from the upper side of the stem plus half the thickness, and the constant is determined so as to satisfy best the observations; the average difference between the observed and computed values of N is a little less than 1 per cent. The Ut_3 and Sol_3 are thinner than the others and so relatively shorter, and were omitted in determining the constant; besides Ut_3 is not uniform in thickness.

$$(6)\quad N=82550d\div l^2.\quad\text{Therefore}$$

$$(7)\quad P=16540N^{\frac{3}{2}}.\qquad(8)\quad V=\tfrac{1}{2}\times16540N^{\frac{3}{2}}a^2.$$

$$\text{For } Ut_2=128\ d.\ v.\qquad(9)\quad V_2=\tfrac{1}{2}\times23{\cdot}94\times10^6a^2\text{ Ergs.}$$

For any other fork of this series of harmonics of Ut_2, if h represents the number of the harmonic,

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* Comptes Rendus, lxxix, 1001, 1069.

(10) $2V_h^a = 23{\cdot}94 \times 10^6 h^{\frac{3}{2}} a^2$ Ergs. = Potential Energy in *both* prongs

when the end of the prong is bent a centimeters.

If the fork is vibrating without giving over-tones, twice in each period its energy is wholly potential, and we may use equation (10) to compute it. The energy of the vibrating rod will actually be a very little greater than of the rod bent into the form of the elastic curve having the same maximum ordinate; for on comparing the curve of a bar fixed at one end given by Rayleigh (i, 231) with the elastic curve, we see that the latter lies everywhere, except at the ends, a very little nearer the line of equilibrium; but the difference in energy is too small to be considered here. The error is greater if the prong ought to be treated as the free end of a straight rod with two nodes, as a comparison of Rayleigh's Figs. 28 and 31 will show; but the latter assumption is certainly further from the truth than the former one.

I have used the method given above for finding the energy, rather than the one taken by Rayleigh (i, 202); because for experimental purposes it is more convenient to express the energy in terms of the amplitude rather than of the radius of curvature.

Experiments.—The forks named in table I were removed from their boxes, and supported firmly under a microscope carried by a dividing-engine with a millimeter screw, the plane of bending of course being parallel to the screw, and the prongs vertical: a cord passed from each prong over a pulley to a scale-pan in which the weights were put: the microscope was focussed on a point on the end of the prong, and then the distance measured between two points, one on each prong, as weights were added in the pans; this gives $2a'$ (a' is expressed in mm., while a was taken in cm.). But as the microscope had to be moved about 20 mm. to pass from one point to the other, a more rapid method was devised, that proved more accurate: a small block of wood was screwed into the end of the microscope-tube carrying at its lower end a brass plate bent thus:

; the distance bc was about 30 mm.; a wire ran from the plate through a telegraph sounder to a battery whose other pole was connected to the fork. A series of observations consisted in observing the difference of reading of the screw when electric contact was made at b and then at c, usually 1 or 2 mm.; equal weights were then put in the two pans, 1, 2, 3 and 4 kg., and the screw readings again observed; their difference is less than before by $2a'$; at least two determinations were made, and the friction of the pulleys eliminated by taking readings when the pans were pulled down as far as they would remain, and when raised as high as they would remain. The value of the quotient $2a'\div W'$ is given in table I, col. 5; a' is here in mm. and W' in kg. The results under Mi_3 which are in [ ] were obtained by drawing the prongs together instead of separating them, as in all other cases; these are less reliable, because of the friction of the strings which had to cross each other. A small correction must be made to these results in col. 6; for the forces were not applied at the end, but usually 1½—2 mm. below it, and the measurement was made above the point of application of the force, usually 1 mm. from the end. It seems hardly necessary to explain the details of the calculation, as it is easily made from the equations of the elastic curve, and of the angle between it and its tangent at the point where the force is applied. In only two cases does it amount to ·010 mm.; on the Ut_2 the forces were applied 16 mm. below the ends of the prongs.

TABLE I.

1	2	3	4	5	6	7	8	9	10	11	12	13
Fork	Length.	Method.	W′	$2a'\div W'$.				P′= 2W′÷2a′	P Obs. C.G.S.	P From(7)	Dif. ÷P(10)	2V= Energy Ergs.
				Obs.	Mean.	Cor.						
	Mm.		Kg.	Mm.			Mm.	Kg. Mm.	10^7 ×	10^7 ×		^{8}×
Ut_2	203	Elec.	2	·772								
			3	·772	·772	·125	·897	2·22	2·18	2·39	−·10	450
Ut_3	141	Micr.	1	·325								
			2	·323								
			3	·327								
			4	·323								
			4	·324								
			2	·315								
			4	·316	·322	·005						
		Electr.	2	·320								
			4	·320	·320	·009	·328	6·10	5·98	6·77	−·13	1236
Mi_3	130	〃	2	·217								
			4	·215	·216	·007	·223	8·97	8·80	9·47	−·06	1820
		[〃	2	·227								
			4	·235	·229	·011	·240	8·33]				
Sol_3	116	〃	2	·174								
			4	·172	·173	·007	·180	11·11	10·9	12·46	−·14	225
Ut_4	104	〃	2	·103								
			4	·098	·101	·004	·105	19·0	18·7	19·16	−·02	386
Sol_4	84									36·0		750
Ut_5	72	〃	2	·026								
			4	·034	·030	·002	·032	62·5	61·3	54·2	+·12	1267

All the observations were made at temperature of 15°–20° C. The other columns in table I give

9. $P'=W'\div a'=2\div(2a'\div W')$ where the accents indicate kg., mm. units.

10. P in C. G. S. units. $P = P' \times 981 \times 10^4$.
11. P computed from equation (7).
12. The difference between 10 and 11 divided by P in col. 10.
13. The energy in both prongs of a vibrating fork when its amplitude is expressed in divisions of a micrometer; 220 div. $= 1$ cm. The energy $= 2 \times \frac{1}{2} P \left(\frac{z}{220} \right)^2$, where P is to be taken from column 10, and z is the amplitude.

Loss of Energy from the fork.—The usual equation for the motion of a body in a resisting medium is

$$(12) \quad u = A' e^{-\frac{1}{2}\kappa t} \sin \left\{ \sqrt{p_2 - \tfrac{1}{4}\kappa^2}\, t - \alpha \right\},$$

where x is the coefficient of damping, and t is the time measured from the instant when $u=0$ (Rayleigh i, 37); $A' e^{-\frac{1}{2}\kappa t}$ will correspond to a' in our equations. The energy at any instant is equal to $P' a'^2 = P'(A' e^{-\frac{1}{2}\kappa t})^2$, being sensibly constant throughout any one period. Therefore

× $$(13) \quad \text{Kinetic energy} = 2T = 2P' A'^2 e^{-\kappa t} = 2P'' A^2 e^{-\kappa t} = P'' z^2 = 2V,$$

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p. 49
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where P″ is the number in table I, column 10, divided by $(220)^2$, and A is the amplitude (in micrometer divisions) when $t=0$; 2V is given in column 13; z is the amplitude in micrometer divisions at the time t.

The rate of loss of energy =

$$(14) \quad \frac{2dT}{dt} = +2\kappa P'' A^2 e^{-\kappa t} = +2\kappa T = 2\kappa V.$$

We must then determine x from the equation $z = A e^{-\frac{1}{2}\kappa t}$, whence $x = 2$ Nap. log. $\frac{A}{z} \div t$; it is more convenient in this case to take the second as the unit of time, rather than the period of vibration as is sometimes done in experiments on damping. For the experiments an upright post about two inches square fixed to a solid base was taken, and to it the resonance case, on which the fork was firmly screwed, was clamped by a carpenter's clamp. Above, a bar held by another clamp supported the brass arm carrying the microscope tube in a vertical position; a little black antimony powder* was gummed on the tip of the fork and these minute particles furnished fine bright points under the microscope, having often a breadth less than $\frac{1}{200}$ mm. The magnifying power used was about 45, and the divisions of the eye-piece micrometer (a glass scale ruled to $\frac{1}{5}$ mm.) were, as already stated, almost exactly equivalent to $\frac{1}{22}$ mm. The adjustments of the apparatus were easy and direct, and the stability almost perfect. To measure the time a stop watch was used (an "Auburndale timer"), held in the left hand, while the right hand was free to use the bow and make records; the pointers gave directly $\frac{1}{8}$ second. Thus the time was found which was necessary for the amplitude to decrease from z' to z''. An example will indicate in a general way the accuracy of the method. Fork Ut$_3$; $z'=12$ div., $z''=6$ div.; time $=9\frac{5}{8}$, $9\frac{7}{8}$, 10, 11, $10\frac{1}{8}$, $9\frac{7}{8}$, $9\frac{5}{8}$, $10\frac{1}{8}$, 10, $9\frac{7}{8}$, $10\frac{4}{8}$, $10\frac{3}{8}$; mean 10·08 sec. These were not consecutive observations, and several points were taken. When z is large the point must be comparatively broad to be seen; while if z is small the finite breadth of the "point" and of the lines on the scale leads to uncertainty.

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* Suggested by Terquem, Phil. Mag., March, 1874; C. R. lxxviii, 125, 1874.

Table II gives for one of the forks used the mean value of κ for a number of intervals. It is an unexpected fact that κ varies so much with the amplitude; only Poske,* so far as I have discovered, refers to it, and then incidentally. It is a proof that the resistance to the motion of a fork is not expressed by a term proportional to the first power of velocity simply, but the second and perhaps higher powers must come in; the equation of motion would be $\ddot{u}+\kappa\dot{u}+\lambda\dot{u}^2+n^2u=0$. But this is not integrable, and even the results which Poisson gives for the case when $\kappa=0$ shed no light on this case; we must therefore determine κ empirically as a function of z and introduce the corresponding value of κ into equation (14).

TABLE II.

1	2	3	4	5	6	7	8	9
Fork.	No. obs.	z'	z	$\frac{z'-z}{2}$	t	κ obs.	κ comp.	Dif.
Sol$_4$	5	10	5	7·5	4·70	·295	·295	+ ·000
	4	9	3	6	9·05	·243	·253	– 10
	2	9	2	5·5	14·30	·210	·239	– 29
	2	8	5	6·5	3·19	·293	·267	+ 26
	11	8	4	6	5·34	·259	·253	+ 6
	4	8	3	5·5	8·19	·239	·239	0
	4	8	2	5	12·8.	·216	·225	– 9
	4	6	3	4·5	6·31	·220	·211	+ 9
	2	6	2	4	11·0	·200	·197	+ 3
	6	5	3	4	5·14	·199	·197	+ 2
	5	5	2	3·5	10·05	·182	·183	– 1
	3	4	2	3	8·17	·170	169	+ 1
	3	4	1	2·5	17·75	·156	1·55	+ 1
	55							
							13)	+ 48 – 49
								± ·008

It will be seen that the observations are fairly represented by putting

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* Pogg. Ann., clii. 470.

$$(15)\quad x=a+b\frac{z'+z''}{2};$$

$\frac{1}{2}(z'+z'')$ is the average of the amplitudes at the beginning and end of the time t; x cannot be expressed as a simple function of the time; so the equation corresponding to (12) can only be solved by successive approximation. Equation (15) leads to the expression $x=a+bz$, for the coefficient of damping corresponding to the amplitude z; therefore instead of (12) we have when t is small, putting z' for A,

$$(16)\quad u=z'e^{-\frac{1}{2}(a+bz')t}\sin\left\{\sqrt{p^2-\tfrac{1}{4}(a+bz')^2}\,t\right\}:$$

if we wish to make t more than a few periods replace z' in the exponent by $\frac{1}{2}(z'+z'')$, where z'' is the amplitude at the time t. Obviously

$$(17)\quad z''=z'e^{-\frac{1}{2}\{a+\frac{1}{2}b(z'+z'')\}t}.$$

As I do not find any discussion of such an equation as (16) it is well to show what is involved in it. Differentiating twice and dropping the accent from z' we get

$$(18)\quad \ddot{u}+a\dot{u}+bz\dot{u}+p^2u=0.$$

Represent the mean value of $\dot{u}$ during a quarter period by $(\dot{u})$; of $\dot{u}^2$ by $(\dot{u}^2)$; and the mean value of $(\dot{u})\dot{u}$, that is, the mean value of the product of the velocity at any time and the mean velocity, by $((\dot{u})\dot{u})$; and neglect the second term under the radical in (16), since in no case for the forks of the present series does it equal $p^2\times10^{-7}$. The familiar methods give*

$$(19)\quad (\dot{u})=\pm\frac{2p}{\pi}z. \qquad (20)\quad ((\dot{u})\dot{u})=\pm\frac{8}{\pi^2}(\dot{u}^2)=\pm\frac{8}{\pi^2}\times p^2\frac{z^2}{2}.$$

Whence

$$(21)\quad \ddot{u}+a\dot{u}\pm\frac{\pi b}{2p}(\dot{u})\dot{u}+p^2u=0.$$

The mean value of the third term =

$$(22)\quad \pm\frac{4b}{\pi p}(\dot{u}^2)=\pm\frac{2b}{\pi p}z^2;$$

the upper sign is to be taken when $\dot{u}$ is +, i. e. when the angle pt is in the first or fourth quadrants, otherwise the lower sign. In the same way then as we may say that $(\dot{u})$, the mean value of $\dot{u}$, and $(\ddot{u})$ vary as z; so the mean value of the third term varies as z^2, and therefore both as $(\dot{u})^2$, the square of the mean velocity, and as $(\dot{u}^2)$ the mean square of the velocity; not, however, as the square of the variable velocity. If $b=0$ of course we get the usual equation of harmonic motion in a resisting medium.

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* In strictness equations (19) and (20) are true only when $a=b=0$; but the error here is very small.

Turn now to the results of the experiments.

Table II is a specimen of the table made out for each of the forks used; it gives the mean time required for the amplitude to decrease from z' to z'', the number of observations from which this mean is deduced, the resulting value of x, x computed from the equation $x = \cdot 085 + \cdot 028 \frac{z' + z''}{2}$, and the differences between the observed and computed values. Where these errors are large it will be seen that few observations were taken or z'' was small.

Table III gives an abstract of table II and similar abstracts for the other forks; it gives the number of ratios observed, the number of observations, the maximum value of z' and the minimum value of z'' used in deducing x; the longest time observed, the coefficients a and b, and the average difference without regard to sign between the observed and computed values of x as found from such data as table II, col. 9. I think we may conclude that equations (16) and (17) satisfy the observations: if any term in the exponent is to be sought depending on $\frac{1}{3}\frac{z'^3 - z''^3}{z' + z''}$, very much more refined methods than these must be devised.

TABLE III.

1	2	3	4	5	6	7	8	9
Fork.	No. ratios.	No. obs.	Max. z'	Min. z''	Max. t.	a	b	Av. dif.
[illegible]	4	[illegible]	[illegible]	[illegible]	[illegible]	·150	·003	±·006
[illegible]	[illegible]	[illegible]	15	2	20·5	·065	·008	±·002
[illegible]	18	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	·028	±·007
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	·122	·022	±·002
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	±·012
[illegible]	4	[illegible]	5	[illegible]	[illegible]	·237	[illegible]	±·006
[illegible]	[illegible]	[illegible]	4	[illegible]	[illegible]	·112	[illegible]	±·007
[illegible]	[illegible]	[illegible]	4	[illegible]	[illegible]	[illegible]	[illegible]	
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	·225	·062	±·008

B[illegible] w[illegible] [illegible] may be placed in the res[illegible]ts, [illegible] considerable change in the [illegible] change x for any given [illegible]:

[illegible] $x = \cdot 150 - \cdot 003\ z$.
[illegible] $x' = \cdot 048 - \cdot 003[illegible]\ z$.
[illegible] $x = \cdot 065 - \cdot 008\ z$.
[illegible] $= \cdot 075 - \cdot 004\ z$.
[illegible] from 5 to 3 = 9·12 sec.
[illegible] 5 to 3 = [illegible] sec.
[illegible] 5 to 3 = [illegible] sec.
[illegible] $= [illegible] - \cdot 02[illegible]\ z$.
[illegible] $x = \cdot 113 - \cdot 02[illegible]\ z$.

Even when the conditions were similar, so far as known, considerable differences were found in x for the same values of z', z'', as table III shows for two forks; the last line has been used in the succeeding computations when the table shows more than one set of observations. I suspect that a part of the variability of b may be due to a different strength of the overtones at different times.

The numbers in column 8, table III (taking the last line for Sol_4 and Ut_5), show such a regular increase as to render it at least plausible that the term containing b depends on the resistance of the air to the faces of the prongs. Schellbach* has shown that the resistance of the air to discs varies as v^2, even when v is as low as 17·1 cm. per sec. In these forks the mean velocity of the tip of the prongs will be $\frac{2}{\pi}\times\frac{2\pi zn}{220}=\cdot01818zn$ cm. per sec., where z is given in micrometer divisions, and n is the vibration-frequency. If the forks have the amplitudes given in table IV, column 7, the least value of ·01818zn is 18·6 cm. per sec., while for most of the forks the value is about 37; the velocities for an ordinary vibration are therefore higher than in Schellbach's experiments, and so we may assume that the air-resistance varies as v^2. We may also assume that the resistance varies directly as the length of the fork when other conditions remain the same:

$\therefore \mathrm{R}=(\cdot01818)^2z^2n^2l^{-1}\times$ a constant; by Mercadier's formula (6) $nl^2=$ a constant.

(22) $\therefore \mathrm{R}=kz^2n^{\frac{5}{2}}$, where k is a constant depending on the breadth, thickness and modulus of elasticity of the prongs, the form of the curve which the vibrating prong assumes, the density and viscosity of the air, and perhaps on other things; but for the forks of this series it is considered constant. Accordingly b in table III, col. 8, should vary as $n^{\frac{3}{2}}$.

Taking $\mathrm{Ut}_2=1$ we get the two series:

$n^{\frac{3}{2}}=$	1·	2·83	5·2	8·	14·7	22·6
$b=$	·003	·008	·028	·022	·044	·062
$1000b\div n^{\frac{3}{2}}=$	3·	2·8	5·4	2·7	3·	2·7

Considering the difficulty of determining b with accuracy, the close agreement of the quotients in the last line seems remarkable. No change has been made in table III since these calculations were made; so there has been no possibility of biasing the judgment in favor of this regular increase of b.

The energy that the fork loses is expended in these ways:

1. In heating the fork, and the wood of the resonance case; 2. In communicating tremors to the support; 3. In producing

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* Pogg. Ann., cxliii, 10.

a sound wave. It is doubtful if the first and second can ever be separated from the total; however large a fraction of the total 1 may be, it is very small absolutely; all the energy that can be given to a fork by a bow would not suffice to heat the fork $\frac{1}{1000}$° C. in the most favorable case in table IV (Ut_5, col. 6). Table IV is drawn up to show roughly the amount at our disposal in these forks. Col. 2 is the same as col. 13, table I; 3 gives approximately the maximum value of z that can be given by a bow; 4, the corresponding energy; 5, the corresponding value of κ obtained by extrapolation, since accurate observations could not be made with such values of z; 6, the maximum rate at which energy is given up. Cols. 7–10 give similar data for an ordinary or average value of z. Mayer* finds from the heating of a bit of rubber between the prongs of Ut_3 fork vibrating electromagnetically, that 54 foot-grains of work are given up to the rubber in 10 seconds: this is equal to 10500 ergs. per sec.; the data in table IV would give this value of $2V\kappa$ when z is a little less than 9 div., or about 0·4 mm.—a very satisfactory agreement.

TABLE IV.

1	2	3	4	5	6	7	8	9	10
Fork.	2V	Max. z	Max. 2V	κ	2Vκ	Av. z	Av. 2V	κ	Av. 2Vκ
	z^2 ×		Ergs. 10^3 ×		Ergs. sec. 10^3 ×		10^2 ×		Ergs.
Ut_2	450	30	405	·27	110	8	290	·20	5800
Ut_3	1235	20	490	·225	110	6	445	·11	4900
Sol_3	2250	13	380	·45	170	5	560	·225	12600
Ut_4	3860	9	310	·32	100	4	620	·22	13600
Sol_4	7500	7	370	·49	180	3	675	·35	24000
Ut_5	12700	6	456	·61	280	2	510	·35	18000

The ultimate object for which these experiments were begun was to determine how much energy was needed to cause sensation, or rather how much energy passed through 1 square cm. at the limit of hearing: for it was believed that the "efficiency" of the tuning fork would be higher than of other sources of a sound-wave.

The forks were taken out of doors to a field where the listeners might be [illegible] feet distant from the fork. This was sounded on the frame described above, and the amplitude observed when the different listeners shouted "now." The separate observations were quite inconsistent, considering that passing teams and a rising wind increased very much the difficulty of observation. Sometimes the observers changed places, or the fork was directed toward different parts of the field. The observation

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* [illegible]

Sol$_4$ was the last and was interrupted by a shower, so that confidence is to be placed in it than in the others; but all give results greatly above those to be obtained under more)rable conditions; inconsistent as these results are, they are of the same order of magnitude, and may deserve publication ause, so far as I can find, no similar work has been published. 'able V gives the results of these field observations; the nber of observations for each listener is given, and the an of the values of z observed when he shouted "out." arly all through the scale the observer Y, who is a musician, wed a higher sensibility than the others, whether the wind toward him or from him.

TABLE V.

1	2	3	4	5	6	7	8	9	10
Fork.	Obs.	50 ft.		100 ft.		200 ft.		300 ft.	
		No. obs.	z	No. obs.	z	No. obs.	z	No. obs.	z
Ut$_2$	Y	5	3·4	8	6·8				
	T	5	3·9	5	7·0				
	E	5	3·5	4	7·0				
	G	5	4·	5	7·0				
			3·7		7·0				
Ut$_3$	Y			1	1·	4	2·4	4	3·9
	T			1	1·5	4	2·5		
	E			1	1·5	4	2·4	4	3·7
					1·3		2·5		3·8
Sol$_3$	Y					6	1·2		
	T					6	1·8		
	E					6	1·5		
							1·5		
Ut$_4$	Y					12	2·0		
	T					8	2·1		
							2·0		
Sol$_4$	Y					6	1·5		
	T					6	1·5		
							1·5		
Ut$_6$	Y					7	·70		
	T					3	.75		
							·72		

a table VI are given the mean values of z of all the listeners ether, and the corresponding values of 2V, x and 2Vx as in e IV. The energy expended per second, whose amount is en in col. 5, passes out through a hemispherical shell whose us is given in col. 7; this statement assumes that the ground ects all the energy that reaches it, which is the assumption tly made by Rayleigh* in his oft-quoted experiment.

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* Proc. R. S., xxvi, 248.

(23) $\frac{2V\kappa}{2\pi D^2}=S=$ energy per sec. passing through the unit of surface at the limit of hearing; or for 200 ft.

$$S=\frac{2V\kappa}{2\pi(200\times 30{\cdot}5)^2}=2V\kappa\times 4{\cdot}3\times 10^{-9} \text{ ergs. per sec. per square cm.}$$

The values for S are given in col. 6. They seem to indicate that the ear is most sensitive to notes in the middle octave; but with regard to Ut_2 the listeners found it so near the pitch of the ever-present noises that it was more easily lost than other notes.

TABLE VI.

1	2	3	4	5	6	7
Fork.	z	$2V$	κ	$2V\kappa$	$S=\frac{2V\kappa}{2\pi D_2}$	Distance.
		$10^2\times$		Ergs. per s.	$10^{-8}\times$	
Ut_2	3·7	62	·195	1200	8300	50 ft.
	7·0	220	·20	4400	7600	100 "
Ut_3	2·5	77	·085	650	280	200 "
	3·8	178	·09	1605	310	300 "
Sol_3	1·5	51	·12	610	260	200 "
Ut_4	2·0	150	·17	255	110	200 "
Sol_4	1·5	170	·30	5100	2200	200 "
Ut_5	·72	63	·26	1640	710	200 "

The Ut_2 fork was mounted *before* a resonance box, and fitted to be driven by an electro magnet; Ut_3, Sol_3 and Ut_4 were mounted on boxes of the usual form open at one end; the Sol_4 and Ut_5 were open at both ends. The Ut_2 was heard much better in front of the box than behind, but as there were four observers, and those before and behind the box exchanged places, any error from this difference is eliminated; no difference was detected with the other forks.

Two in-door experiments may be given: the Ut_2 was taken near one end of a long hallway, and, being turned in various directions, the amplitude was noted when the sound ceased to a listener about 50 ft. away: here we cannot apply the law of inverse squares; the best thing is to assume that all the energy passes through every transverse section of the hall, although we know that some was absorbed by the end wall, and some escaped by a stairway:

$$2V\kappa=190 \text{ and area } =19\,\overline{\mathrm{m}}^2 \ \therefore\ S<100000\times 10^{-8}.$$

Again the Ut_3 fork was heard about 2 m. distant when the amplitude was less than $\frac{1}{12}$; as the fork was near a wall, assume that the energy passed out through a quarter of a sphere; and as the ear was at a point of maximum loudness, assume that the energy per second at that point is four times that due to a uniform distribution of the energy:

$$2V\kappa < 0{\cdot}52\,;\ S < 4\frac{0{\cdot}52}{\pi \times (200)^2}\,;\ S < 1700 \times 10^{-8}\,;$$

table VI gives $S = 300 \times 10^{-8}$.

Some experiments were made bearing on the question of the division of the energy as already referred to; they indicate that with the Ut_3 fork only about $\frac{1}{16}$ of the total energy is used for the sound-wave; but the observations are too few to determine whether the theory used in their discussion is valid or not; so this matter must be deferred till another time.

The only other experiments that I know of with which to compare the values of S in table VI are three in number, from which I have computed the values of S as follows:

Töpler and Boltzman.* closed organ pipe, $n=182$	$S = 10000 \times 10^{-8}$
Rayleigh.† open pipe, $n=2730$	$S = 4500 \times 10^{-8}$
Allard,‡ bells, steam syren, etc., at sea, n from 400 to 1500	$S = n \times 4{\cdot}3 \times 10^{-8}$

Following Rayleigh's formula we get the maximum velocity of the air particles at the limit of hearing, thus: $v^2 = S \div \frac{1}{2}a\rho$, where $a =$ the velocity of sound $=34000$ cm. and $\rho = {\cdot}0013 =$ density of air; $\therefore\ v^2 = S \div 22{\cdot}1$; the amplitude of the vibration of the air particle $= x = v \div 2\pi n = \sqrt{S} \div 30n$. The smallest value of x to be obtained from table VI is 70×10^{-8} cm. for Ut_4. The formula computed from Allard's data would give for $n=512$, $x = 31 \times 10^{-8}$ cm. Rayleigh found for $n=2730$, $x = 8{\cdot}1 \times 10^{-8}$ cm.

But the fuller discussion of these matters must be postponed to another part of this paper.

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[*] Pogg. Ann., cxli, 321, 1870.

[†] Proc. Roy. Soc., xxvi, 248, 1877.

[‡] Comptes Rendus, xcv, 1062, 1882. I have not seen his longer memoir.

ART. XXIII.—*The Decay of Rocks Geologically Considered*; by T. STERRY HUNT, LL.D., F.R.S.

[Read before the National Academy of Sciences at Washington, April 17, 1883.]

CONTENTS OF SECTIONS.—1, 2. The supposed relation of rock-decay to climate; 3, Decomposition of silicated rocks; 4, 5, Studies of Pallas, Fournet, J. F. Dana, Webster, Blake; 6, Of Hartt in Brazil; 7, Of Shepard in Connecticut; 8, 9, Whitney, Pumpelly and Dawson on decay of limestones; 10, A sub-aërial process; 11, 12, Its antiquity and its relation to glacial erosion; 13, Decay in the Blue Ridge; Hunt, Burbank; 14, 15, Chemical significance of decay of aluminous silicates; 16, Analyses of decomposed feldspars; 17, 18, The question in relation to eozoic rocks; 19, Bowlders in crystalline schists; 20, 21, Decayed gneiss of Hoosac Mountain; 22, Of the South Mountain, Penn.; 23, At Atlanta, Ga.; density of decayed rocks; 24-26, Cupriferous pyrites; its decay; iron and copper ores; 27-30, Limonites of the Appalachian valley; tertiary limonites (foot-note); 31-34, Lesley, Jackson, Frazer and Lyman on limonites; 35, 36, W. B. Rogers on formation and segregation of siderite; 37, 38, Limonites from serpentine of Staten Island; 39, Pumpelly on decay of petrosilex-porphyry in Missouri; its pre-Cambrian antiquity; 40, Of gneisses in Wisconsin and Minnesota; White, Winchell and Irving; 41, Rock-decay in Sweden and in Scotland; 42, Post-Cambrian decay of igneous rocks; 44, 45, Decay of auriferous gravels in California; influence of carbonic acid; 46, Slight decay in post-glacial times; effects of changes of temperature (foot-note); 47-49, Pumpelly on the geological relations of rock-decay; 50-52, Studies of Reusch in Corsica; 53, Conclusions.

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§ 1. The subject of the decay of rocks has not yet received from geologists all the attention which it merits, and there still appear to be misconceptions with regard to it which warrant us in reviewing some points in its history. Professor F. H. Storer,[1] in a recent notice of a suggestion of Nordenskiöld's as to the liberation of gems through the decay of the feldspathic rocks in which they are often contained, cites with approval the opinion of Professor Stubbs of Alabama that "the decomposition of these rocks in southern latitudes has proceeded much faster than with the same rocks in higher latitudes," a "condition which can be accounted for, to a large extent, by climatic influences." The cold and frost now prevailing in northern regions are supposed by him to retard the action of atmospheric waters, regarded as the chemical agent of this process of decay.[2] These views implying that the process is one belonging to the present time, are accepted by Professor Storer, who writes of the more active and thorough-going disintegration which occurs in these southern regions.

§ 2. That the presence in the northern hemisphere of a mantle of softened material from the decay *in situ* of crystalline rocks is more common at the surface of these in low than in high latitudes, where it is often entirely absent, is a familiar fact, but it will, I think, be made evident that present climatic differences have nothing to do with the fact that similar rocks are in one area covered with a thick layer of the products of decay, and in another are wholly destitute of it.

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[1] [illegible]

[2] [illegible] 1878, p. 193.

§ 3. The decay in question is well known to be due to a chemical change of which the predominant mineral silicates of the rock, chiefly feldspars and hornblende, are the subjects, and which results in the removal by solution of the protoxyd-bases, together with a great proportion of the combined silica, leaving essentially a residue of clay, mingled with quartz, garnet, magnetite and such other mineral species as resist the process of decomposition.

§ 4. A memoir, by Fournet, published in 1834,[3] gives many facts regarding the early observations on rock-decay. Its author there describes the wide-spread decomposition of the granites near Pont-Gibaud in Auvergne, a change which Deribier de Cheissac had already shown to be anterior to the deposition of the Tertiary rocks. Fournet, moreover, noticed the similar decay of basalts, phonolites, trachytes, and even obsidians, and described the process of exfoliation by which were left rounded masses of undecayed rock. He cites in this connection the observations of Pallas, who, in his travels in Siberia (1768-1774), noticed hills "that seemed composed of masses heaped together, as it were rounded by decomposition." The view of Werner that the rounded form of masses such as these was due to original concentric structure, was rejected by Fournet.

§ 5. In 1818, Messrs. J. F. and S. L. Dana described a similar phenomenon in the decaying greenstones at Somerville, near Boston, Mass., where the rock was found to be converted by decay *in situ* into nodular masses presenting exfoliating concentric layers of differing degrees of decomposition. These masses rest upon each other, decayed matter filling the interstices.[4] In 1825, T. W. Webster noticed the same example, and explained the formation of bowlders by the exfoliation of the decayed greenstone.[5] Again, in 1858, W. P. Blake described the production of rounded masses both of sandstone and of granite through disintegration. He explains how angular blocks, separated by joints admitting water to all sides, would be "attacked most rapidly on the angles, thus producing a succession of curved faces gradually approaching a sphere," and illustrates the process by figures. He described, moreover, the bowlder-like masses of granite in Placer County, California, lying on an uneven surface of the same rock, "as due to the manner in which the rock decomposes, and not to abrasion." Like Fournet, he rejects the notion of an original concentric structure in the rock.[6]

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[3] Ann. de Ch. et de Phys. [2] v. 225-256.

[4] Mem. Amer. Acad. Sciences, 1st Series, iv, 201.

[5] Boston Journ. Philos. and Arts, ii. 285.

§ 6. Hartt, in 1870, discussed the well-known examples of rock-decay found in Brazil, and called such rounded masses of rock as we have just described "bowlders of decomposition." He moreover noted that the process of decay was there anterior to the supposed glacial action, which had worked over the material of the previously decomposed rocks.[7] Lyell already, in 1849, had pointed out that the Tertiary clays and sands of the southern United States have been derived from the waste of the previously decayed crystalline rocks of the region;[8] and as we have seen, the ante-Tertiary age of the decay in Auvergne had long before been recognized.

§ 7. The account given by Professor Charles Upham Shepard, in 1837, of the origin and mode of occurrence of the porcelain-clays of western Connecticut is remarkable for its exactness and perspicuity. That at New Milford is described as occurring "upon the western slope of an elevated range of granitic gneiss. . . . . In many places the decomposition of the parent rock is so complete as to present the aspect of a secondary deposit; but the prevailing appearance is that of the rock altered in place through the decay of the feldspar and mica. Indeed, the same relative arrangement of the quartz and the altered feldspar is observed in the bed as is presented by these materials in the undecomposed rock. Veins and seams of a perfectly impalpable white clay traverse the rock in various directions, analogous to the veins of feldspar in the granite of the neighborhood." Of a pure white clay in the town of Kent, our author says, "It forms a vein many feet in width, cutting through quartz rock. It owes its origin to a graphic granite, which must have been free from mica." A similar vein of clay is described as occurring in the town of Cornwall, and as including frequent crystals of black tourmaline; the feldspar also being incompletely decomposed.[9]

§ 8. As showing that the process of sub-aerial decay is not confined to silicated rocks, it may be noted that J. D. Whitney described in 1862, the existence in the lead-region of Wisconsin of a layer of red clay and sand, mixed with chert, sometimes thirty feet in thickness, which he showed to be a residuum from the secular decay of several hundred feet of the impure Paleozoic limestones of the region.[10] A like occurrence was afterwards, in 1873, described by Pumpelly, in southern Missouri, where such residuary deposits sometimes attain a thick-

[6] Geol. Recon. of California, pp. 146, 286.

[7] Scientific Results of a Journey in Brazil, pp. 28, 573.

[8] Lyell: A Second Visit to the United States, ii. 28.

[9] Shepard: Report of Geological Survey of Connecticut (1837), pp. 73–75.

[10] Geology of Wisconsin, i. 121.

ness of 120 feet.[11] This process is evidently due to a simple solution of the carbonates of lime and magnesia in meteoric waters.

§ 9. A similar decay is conspicuous along the outcrop of the Auroral limestones and their associated schists in the Appalachian valley, as will be noticed farther on, in § 27, and may also be seen at several points in the Trenton limestone and the Utica shale of the St. Lawrence valley. One of these localities, described by Dr. J. W. Dawson, is at Les Eboulemens, on the north shore of the St. Lawrence, below Quebec. Here, at the southwest base of the high Laurentide hills, the Post-pliocene clays, enclosing marine shells and large gneiss bowlders, are found resting upon a mass of Utica slate, deprived of its calcareous matter, and so soft as to be readily mistaken for the newer clays of the region but for its stratification and its organic remains. This, according to Dawson, had been changed to a great depth by sub-aërial action previous to the period of submergence, during which it was covered with the bowlder-clay.[12] Some facts connected with the decay of the Trenton limestone near Montreal will be mentioned in § 42.

§ 10. It may be said that with the exception of Darwin, who had observed the decay of rocks in Brazil and conjectured that the process might have been sub-marine, all observers have correctly regarded it as sub-aërial. The chemistry of the process was discussed, among others, by Fournet in the paper already cited, and later by Delesse in 1853;[13] also very fully by Ebelmen, who considered the question of rock-decay in its relations to the atmosphere, in two memoirs in 1845 and 1847.[14] The same subject was further considered at some length by the present writer in 1880.[15]

§ 11. Having thus briefly indicated some of the points in its history during the past century, we are prepared to notice in more detail the contributions made to the subject, considered in its geological bearings, during the last ten years. Previous to this, as we have seen, it had been recognized that the process of rock-decay had been in operation not only in pre-glacial but in pre-Tertiary times, and that the resulting material had been the source of Tertiary clays and sands, and even, in certain cases, of glacial drift and bowlders.

§ 12. In a review of Hartt's volume on Brazil, in 1870, the present writer said: "The great wasting and wearing-away of crystalline rocks in former geological periods, of which we have abundant evidence, is less difficult to understand, when we learn that rocks as hard as those of our New York Highlands become [are] even in our own time, under certain conditions so softened as to offer little more resistance to the eroding action of a torrent than an ordinary gravel-bed."[16] Subsequently, in an account of some observations made in North Carolina among the rocks of the Blue Ridge, and presented to the Boston Society of Natural History, October 15, 1873, he expressed the belief that the decay of crystalline rocks was a process of great antiquity; that it had been universal; that the covering of decayed material now seen in the south at one time extended to the rocks of northern regions, from which it had been removed by erosion during successive ages, culminating in the Glacial period at the close of the Pliocene, since which time the chemical decomposition of the surface has been insignificant. From the products of this sub-aërial decay it was then maintained has been derived a great part of the sediments alike o Paleozoic, Mesozoic and Cenozoic times. The permeable natur of the surface-soil formed of highly inclined strata of decaye rocks, affording a natural subterranean drainage, explains, i was said, both the absence of lakes and the comparative permanency of the surface to be remarked in uneroded regions; showing that something more than ordinary aqueous agencies mus have effected the removal of the decayed material."[17]

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[11] Geological Survey of Missouri: Iron Ores and Coal Fields, p. 8.

[12] Dawson: Notes on the Post-pliocene Geology of Canada; from Canadian Naturalist, vol. vi, 1872.

[13] Bull. Soc. Geol. de France, x, 256.

[14] Annales des Mines [4], vii, and xiii.

[15] This Journal, xix, 349; see also his Chem. and Geol. Essays, p. 100.

§ 13. This communication of mine was speedily followed b a paper published in the Proceedings of the Boston Society Natural History for November 19, 1873, by the late Mr. L. Burbank, repeating and insisting upon the same conclusion and, moreover, dwelling especially upon the process of deca (which he also had studied in North Carolina), as a prelimina to the formation of bowlders and glacial drift.

In accordance with the views thus expressed in 1870 a 1873, it was conceived that the power of the usual erodi agents, ice and water, would be inadequate to the removal great areas of rock unless this had been previously softened decay, and in a review of the subject by the present writer, 1873, the conclusion was reached that the decomposition rocks has been "*a necessary preliminary to glacial and eros action, which removed already softened materials.*"[18] Such erosi and denudation would, in accordance with this view, consist the removal of previously decayed rocks, and the forms a outlines of the sculptured surface thereby exposed would be determined by the varying depths to which the process of su aërial decomposition had already penetrated the once firm and solid rock. The basin-like depressions and the hillocks of the eroded surface, not less than the detached rounded masses or bowlders, were thus, as the writer has ever since taught, the results of the previous process of rock-decay.

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[16] The Nation, New York, Dec. 1, 1870.

[17] Proc. Boston Soc. Nat. History, Oct. 15, 1873, and this Journal, vii. also Proc. Amer. Assoc. Adv. Science for 1874, p. 39; and Hunt, Chem. and G Essays, pp 10, 250.

[18] Harper's Annual Record of Science, etc., for 1873, p. xlviii.

§ 14. I had long before this time been led to insist upon the evidences of a wide-spread decomposition of crystalline rocks in very early periods of geological history. In an essay, entitled Some Points in Chemical Geology, published in 1859,[19] and another, on The Chemistry of Metamorphic rocks, in 1863,[20] both reprinted in my volume of Chemical and Geological Essays, I have pointed out the important part played by the protoxyd-bases liberated by the sub-aërial decay of feldspathic and hornblendic rocks. Starting from the conception of a primitive terrestrial crust consisting wholly of crystalline silicated rocks, we are forced to find in such a process of decay the source of all limestones and dolomites, which are derived from the carbonates of lime and magnesia generated either directly, during the process, from the bases previously existing in the state of silicates, or indirectly, by reactions between magnesian and alkaline carbonates formed during the decay, and the calcic salts of the early ocean. The chemical genesis of the lime carbonate must evidently precede its assimilation by organisms. It was, in fact, thus shown, as the result of a great number of observations, that fossil sea-waters (mineral waters), representing the ocean of Paleozoic and even of Mesozoic times, contained large proportions of calcic chloride, such as are required by this theory.[21] The relations of these reactions when "this decay of alkaliferous silicates is sub-aërial," as set forth in 1859 and 1863, will be found discussed at length in the volume of essays above named on pages 23–31, and page 108.

§ 15. I further proceeded at that time to consider the proportions between the alkalies and the alumina in the various characteristic minerals of crystalline rocks, noting the decrease in the former which is seen when silicates like orthoclase and albite are compared with micas like muscovite, and with silicates like cyanite, pyrophyllite and staurolite. The conclusion was then reached that "the chemical and mineralogical constitution of different systems of rocks must vary with their antiquity," and that "it now remains to find in their comparative study a guide to their respective ages;" in which connection a comparison was then attempted between the older gneisses and the newer crystalline schists. A further application of this principle was essayed in 1878, when the progressive elimination of the alkalies from the aluminiferous rocks of the eozoic groups was shown by comparing the mineralogical composition of the Laurentian with the Huronian, Montalban and Taconian crystalline schists.[22]

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[19] Geol. Journ., London, xv, 488–496.

[20] Geol. Soc. Journ., Dublin, x, 85–95.

[21] Hunt: Chem. and Geol. Essays, pp. 41, 108, 117–121.

§ 16. It should here be noted that decayed feldspars, even when these are reduced to the condition of clays, have not, in most cases, lost the whole of their alkalies. This is well illustrated in a series of analyses by Mr. E. T. Sweet, of the kaolinized granitic gneisses of Wisconsin, to be noticed farther on (§ 31). From these analyses it appears that the levigated clays from these decayed rocks still hold, in repeated examples, from two- to three-hundredths or more of alkalies, the potash predominating.[23]

§ 17. It would follow from the considerations advanced above, that the decay of crystalline rocks is an indispensable preliminary to the formation of limestone, and that the earliest silicated rock must have contained no carbonates whatever. There are many reasons for doubting whether this veritable primary system is known to us, but it will be remembered that at the base of the Laurentian, as seen on the Ottawa River, there is a vast and unknown thickness of red and gray granitoid hornblendic gneiss, apparently destitute of limestones, and underlying the great series of somewhat similar gneisses interstratified with limestones with graphite, iron-oxyds and quartzites, which were, as early as 1847, described by the Canadian geological survey as "a separate group of metamorphic strata," characterized by these lithological differences. Logan, however, supposed the two groups to be conformable, and they were in 1854 both included under the common name of Laurentian (afterwards the Lower Laurentian of Logan).

In a subsequent review of the subject in 1878, I designated the lower as the Ottawa group, and the upper group as the Grenville series.[24] It was this latter group which had been carefully studied and mapped by Logan and his assistants, and which has served as the type of the Laurentian system. I have since, in a paper read before the American Association for the Advancement of Science, in 1879,[25] noticed the probable nonconformability of this Laurentian system with the underlying or pre-Laurentian gneiss, and the possible relations of the latter to the fundamental or Lewisian gneiss of Scotland, and the Bojian gneiss of Bavaria.

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[22] Second Geological Survey of Pa.: Azoic Rocks. Rep. E. p. 210.

[23] See for these, Irving on the Mineral Resources of Wisconsin, Proc. Amer. Inst. M. Engineers, vol. viii. p. 305. For other analyses, see Geo. H. Cook. Geol. Survey of New Jersey, Report on Clays, 1878.

[24] Azoic Rocks. pp. 61, 118, 154–156.

[25] The pre-Cambrian Rocks of Europe and America compared. Amer. Jour. Sci., xix, pp. 270, 275, 281; also Geol. Mag., Jan. 1882, p. 38, and Bull. Soc. Géol. de France, x, 26.

§ 18. The existence, in the Laurentian series, of limestones, ot less than that of iron-ores and of graphite, pointing to the xistence of land and of vegetation during the deposition of the aurentian, forces us to conclude to a process of sub-aërial decay f the more ancient gneisses in that far-off period. Such a pro- ess must have been continued in later times to give the materials for the aluminiferous sediments of the newer eozoic groups, and we might therefore hope to find in the latter bowlders or pebbles of more ancient gneisses, such as are met with among the products of sub-aërial decay in later deposits. Remarkable examples of such rounded masses, alike of Montalban, Huronian and Laurentian or pre-Laurentian types are found bundantly in the very ancient pre-Cambrian (Keweenian), con- lomerates on Lake Superior, as I have elsewhere described.[26] ot less striking examples of rounded masses of older gneisses ccur in the Huronian series in many localities, particularly on ake Temiscaming, where are great beds of conglomerate made p chiefly of gneiss bowlders.[27] I have elsewhere noticed a ecimen in my possession which shows a perfectly well-defined d rounded pebble of finely granular white limestone, measur- g an inch in its greatest diameter, enclosed in a laminated rnblendic gneiss from Grafton County, New Hampshire. ices cut from the specimen for the microscope show a strong hesion between the limestone and the quartz and feldspar of e matrix, without however any evidence of chemical change the contact.[28]

§ 19. The rounded masses and pebbles of gneiss found abun- ntly in several localities imbedded in the pre-Cambrian mica- ous schists of the Saxon Erzgebirge are not less remarkable amples of the same kind. I had in 1881 the opportunity of amining with Dr. Credner a large collection of these, which nsist chiefly of types of various kinds of gneiss resembling ose of the Laurentian series as seen in North America and the Alps. These Saxon mica-schists, with their associated neisses passing into granulites or leptynites, have all the char- teristics of the Montalban or newer gneissic series of North merica and of the Alps, to which I have elsewhere compared em in two communications[29] wherein are noticed the above- entioned conglomerates, which had been previously studied much detail by Sauer[30] in 1879. No one who sees these cumulations of rounded masses of gneiss and other crystalline cks entering into conglomerates at the various horizons above med, can fail to be struck with their close resemblance with those which are to be found either in the glacial or other modern deposits, or lying *in situ* as undecayed rounded masses in the midst of decomposed rocks. It is difficult to resist the conclusion that these rounded masses of the eozoic ages must have been formed under conditions not unlike those which gave rise to their more modern representatives.

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[26] Hunt, Azoic Rocks, pp. 78, 230.

[27] Geology of Canada, 1863, p. 50.

[28] Bull. Soc. Géol. de France, [3] x. 27.

[29] Geol. Magazine, Jan. 1882, p. 39, and Bull. Soc. Géol. de France, x, 26.

[30] Zeitschrift f. d. ges. Naturwiss, Band lii.

§ 20. The various considerations above presented thus led the writer in 1873 to assign to the beginning of the process of rock-decay an antiquity compared with which the time that has elapsed since the drift-period is to be regarded as of short duration. It was, however, then suggested by him that a climate and atmospheric conditions unlike those of modern times might have favored the process in the earlier ages. Further evidence was soon forthcoming both of the former spread of this decay over northern regions and of its great antiquity.

In 1874 I was called to examine the condition of the great tunnel then recently opened through the Hoosac Mountain in western Massachusetts, my report on which was published by the General Court of the State ;[31] while a note on the observations therein made which have a bearing on the present inquiry, was presented to the American Institute of Mining Engineers in October, 1874.[32]

§ 21. As there explained, the gneissic rock of Hoosac mountain, at the west end of the tunnel, 700 feet above the sea, is completely decayed, the feldspar being converted into kaolin for a distance of several hundred feet eastward along the line of the tunnel. The gneiss on the crest of the mountain, 2,000 feet above the sea, and on the eastern slope, on the contrary, wherever exposed, presents the rounded surfaces common throughout the region, often marked by glacial striæ, and without any appearance of decay. The softening and decomposition of the highly inclined strata of gneiss in the tunnel were described as complete for a distance of 600 feet from the west portal, where the floor of the tunnel is 200 feet from the surface, and were partial at 1,000 feet, where it is 230 feet below, while farther in, at 1,200 feet, an included bed of limonite, doubtless of epigenic origin, showed that the solvent and oxydizing action of atmospheric waters had penetrated to a depth of more than 300 feet from the present surface. At the western entrance to the tunnel the gneiss is immediately succeeded by the crystalline limestone and quartzite of the Taconian (Lower Taconic) series, the decayed rocks apparently coming from beneath the limestone. It was evident that this great mass of decayed gneiss at the western base of Hoosac Mountain is but a portion of a once wide-spread mantle of similar materials hich has escaped the action that denuded and striated the sur- ice of the other parts of the mountain.

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[31] House Document No. 9, 1875.

[32] Proc. Amer. Inst. M. Engineers, iii, 187.

§ 22. Numerous examples of similar remaining portions of ecayed feldspathic rock have been observed farther southward, s in northwestern Connecticut (described in § 7), and among ie Laurentian rocks of the South Mountain in Pennsylvania, orth of the Schuylkill. One of these decayed portions, at iesholtzville, Penn., was seen in 1875, where a bed of magetite, at that time mined, was found to overlie at a high angle mass of granitoid gneiss completely kaolinized, but apparntly protected from erosion by the incumbent iron-ore.

In another example in the same region, about two miles south f Allentown, Penn., the Primal sandstone was found resting or a little distance on the Laurentian gneiss, here much deayed. Where this had been exposed in a recent cutting (in 875) the reddish feldspathic rock, still retaining its color and ts gneissic structure, though kaolinized, contained numerous bowlders of decomposition," from three to twelve inches in iameter, consisting of undecayed gneiss, the laminated strucare of which was clearly continuous with that seen in the nclosing decayed mass. These bowlders, still *in situ*, spheridal in form, and often with pitted surfaces, are identical with iose found in the drift near by, on the southeast slope of the ill, and are very different in outline from the half-angular rms of adjacent sandstone blocks. This gneiss rock, lying cayed in place, would, unless examined in fresh cuttings, be adily mistaken for the drift of the vicinity, which has evintly been derived from it.

§ 23. In my earlier notices of the decayed Montalban rocks the Blue Ridge in North Carolina, I had described a mantle from fifty to one hundred feet or more of decayed material, ut this according to the late Professor W. B. Rogers, somemes exceeds two hundred feet, a thickness approaching to iat observed at the western base of Hoosac Mountain. I have nce noticed the decay of the Montalban rocks near Atlanta in eorgia, where, with local exceptions of undecayed areas (as Stone Mountain) the decomposition is more or less comlete, in many places, to a depth of fifty feet. Here, as elsehere, the more massive rocks include nuclear masses of undeayed material. The decayed highly hornblendic gneiss of tlanta, though still retaining considerable coherence, has lost bout two-thirds of its weight, the specific gravity of unchanged ortions being 2·97—3·08, while that of the decayed material s reduced to 1·20, and even, for some specimens, to less than ·0.[33] The decomposed gneiss in this region is, in some cases, ufficiently coherent to furnish blocks for certain purposes of construction, such as the walls of rude chimneys, but at the surface it readily disintegrates, yielding a strong red soil, often used as a brick-clay. The decayed mica-schists of the Montalban series, which still retain their micaceous aspect, have been called hydro-mica schists, though distinct from those of the Taconian, with which they have been confounded.

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[33] Azoic Rocks, p. 250.5

§ 24. The relations of the large deposits of cupriferous iron-pyrites found in the rocks of the Blue Ridge, to the general process of decay, was discussed by the writer in 1873,[*] after a study of the copper-mines opened in Carroll Co., Va., in Ashe Co., N. C., and in Polk Co., Tenn.[*] These ore-deposits were described as in each case in rocks of the Montalban group—the newer gneisses and mica-schists—and as constituting veins or lenticular masses of posterior origin, consisting essentially of pyrite, pyrrhotine and chalcopyrite. The agent which kaolinized the enclosing rocks, also oxidized the sulphurets, removing the sulphur and the copper, and converting the residue into limonite, which, in a vertical lode in Ashe Co., N. C., was found to extend to depths of from forty to seventy feet. Beneath the oxidized portion is found in all cases the unchanged pyritous mass, seldom carrying more than four or five hundredths of copper. The limonites thus generated were for some years smelted for iron, both in Virginia and in Tennessee, before they were discovered to be *gossans* at the outcrops of cupriferous pyrites-lodes. Between the unchanged pyrites and the limonite there is often found in favorable conditions an accumulation known as black ore, consisting of imperfectly crystalline sulphurets, rich in copper, and sometimes approaching to bornite in composition, occasionally with red oxide and native copper, the whole doubtless reduced from the oxidized and dissolved copper brought from above.

§ 25. The crystalline Eozoic rocks of various ages in the more northern parts of the continent contain, as is well known, many deposits of cupriferous pyritous ores, both in veins and beds which, like the enclosing strata, are undecayed, showing that the process of oxidation, like that of kaolinization, has been a very gradual one, going back to remote ages. We have seen from the observations in the southern United States that the oxidation of the sulphids, their conversion into limonites, and the removal therefrom by solution of the copper went on *pari passu* with the decay of the including rocks, and hence preceded their erosion. The copper thus dissolved was, as I have suggested, again deposited in rocks at the time in process of formation. The chief part of the iron being left behind, a veritable concentration of the copper would thereby be effected nd we should expect to find it separated on reduction as rich ulphides, or as native copper. In accordance with this view, it was said in an essay on The Geognostical Relations of the Metals, in February, 1873,[35] that certain deposits of such copper-sulphids found chiefly in limestones, probably of Cambrian age (Quebec group), which, in the province of Quebec, as at Acton and Durham, lie along the northwest border of the crystalline Huronian belt, might be formed from "the results of oxidation of the cupriferous beds which abound in the crystalline schists of these mountains, from which the dissolved metal accumulated in basins at their foot," as suggested by Murchison with regard to the cupriferous Permian strata near the crystalline schists of the Ural Mountains. "To a like process," it was said, "we may perhaps ascribe the rich deposits of native copper in the Keweenaw amygdaloids and conglomerates which rest upon the ancient Huronian schists."

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[*] Proc. Amer. Inst. M. Engineers, ii, 123, and this Journal, vi, 305; see also Chem. and Geol. Essays, pp. 217, 236.

§ 26. The farther extension of this view to the Mesozoic sandstones of Connecticut, New Jersey and Pennsylvania, well known to be very often impregnated with copper disseminated in the form of sulphides, sometimes associated with organic remains, is obvious. It is to be noticed that the strata in question are generally deposited directly upon Eozoic rocks, from the ruins of which they were formed, and that these, in our hypothesis, furnished the dissolved copper from which the disseminated ores were derived. If this view be admitted we have farther and independent evidence that the decay of the Eozoic rocks, with that of their contained cupriferous sulphurets, was going on in that pre-Cambrian period in which the Keweenian series was accumulated, and was still active in Mesozoic time.

§ 27. Not less striking examples of rock-decay are seen in the great Appalachian valley, of which Hoosac Mountain, the South Mountain and the Blue Ridge form parts of the eastern rim. Therein, as is well known, large quantities of limonite are mined, from New England to Alabama. This ore, as well as its accompanying manganese-oxide, is clearly of epigenic origin, and is, in most cases, still imbedded in ancient and highly inclined clayey strata derived from the sub-aërial decay *in situ* of the schists which accompany the dolomites and quartzites of the Primal and Auroral (Taconian) series. These ores have been formed by the transformation in place of included masses of pyrites and of carbonates of iron and manganese.[36] The evidences of the pyritic origin of many of these limonites is similar to that for those of the Blue Ridge (§ 24), namely, their association with unchanged pyrites. An example of this is seen in the so-called copperas mine at Breinigsville, near Trexlertown, Penn., long ago described by H. D. Rogers,* where large quantities of pyrites have been mined from the same openings which yield limonite, some of which I found still retaining the imitative forms of the adjacent pyrites, (from which Rogers had inferred a conversion of limonite into pyrites,) while the waters of the mine, like those of others in the region, were charged with sulphuric acid and with iron-sulphate. Another remarkable locality, where pyrites replaces the limonite in depth, was visible in 1875 at Seitzinger's mine, near Reading, Penn., and other similar cases are reported in the vicinity; while at Salona in the Nittany valley the association of pyrites with limonite at this same geological horizon has also been noticed.

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[35] Proc. Amer. Inst. M. Engineers, i. 341.

[36] Azoic Rocks, pp. 201–203.

§ 28. The association of siderite or iron-carbonate with the limonites of the Appalachian valley is well known east of the Hudson in New York and Massachusetts. This mineral is often manganesian and passes into nearly pure rhodocrosite. Examples of the association of siderite with limonite are also seen, among other localities, near Hackettstown, New Jersey, and near Hanover, York County, Pennsylvania. These carbonates, or at least the limonite and manganese-oxide derived from them, are found in close association with pyritous deposits, as we have seen near Trexlertown. In like manner pyrites and siderite as is well known, often occur side be side, in the coal-measures.

§ 29. I have elsewhere considered the change in siderite under the action of oxydizing atmospheric waters, which proceeds like that in feldspathic rocks, from without inwards, and is necessarily accompanied with considerable diminution of volume, which, in the conversion of a siderite of specific gravity 3·6 into a limonite of the same density, would equal 19·5 per cent.

> "The evidences of this contraction may be seen in the structure of the limonite derived from siderite, which often forms a porous or spongy mass. In the case, however, of nodules or blocks of solid ore, the conversion beginning at the outside of the mass, an external layer of compact limonite is formed, and then another within this, and still another, till the change is complete. The void space resulting from contraction is then found between the layers, which are arranged like the coats of an onion; or sometimes wholly at the center, where a cavity will be formed, holding in many cases, more or less clay or sand, the impurities of the carbonate which have been separated in the process of conversion into limonite. In this way are formed the hollow masses sometimes known as bomb-shell ore, [which occasionally include nuclei of unchanged siderite.] Their structu

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* Geology of Pennsylvania, [illegible]

will generally serve to distinguish the sideritic from the pyritic limonites."[38]

In the paper just quoted I have also considered the change of volume which should accompany the conversion of pyrites into limonite, a process generally complicated by the loss of a part of the iron as a soluble sulphate.

§ 30. Portions of the contorted and often highly inclined schistose strata enclosing the limonite ores in the Appalachian valley, are still found but partially decayed, and while some are converted to depths of 100 feet or more into white or variously colored clays, others retain more or less of their original texture. From the presence in some of these of considerable uantities of a hydrous micaceous mineral, having the compoition of damourite, they have been called damourite-slates. There are many reasons for believing that these ancient rocks were thus folded and were decomposed before the deposition f the Trenton and Chazy limestones, which rest upon them in he outlying or western valleys of the Appalachian region, like in Pennsylvania and in Alabama."[39]

§ 31. Prof. Lesley, in discussing the history of the limonites of the Appalachian valley, has fallen into an error with regard o my view of their origin. Referring in 1876 to the opinions xpressed in my paper of 1873, (already noticed in § 13), ouching the decayed crystalline rocks of the Blue Ridge, that 'the iron-oxide from these has been in great part dissolved out by subsequent processes, and was the source of the immense deposits of hydrous iron-ores" in question, he supposes me to "conjecture that the ores lying along the eastern edge of he Shenandoah valley had been washed into it from or across the Blue Ridge." This Lesley properly qualifies as an "absurd conclusion," since it does not explain the origin of the imonites found in the back or central valleys, a hundred miles or more to the west of the Blue Ridge; and declares that had R. S. M. Jackson continued his geological studies, "he would have published a satisfactory refutation of this surface-drainge theory of the brown hematites."[40]

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[38] The Genesis of certain Iron Ores, read before the Amer. Assoc. Adv. Science, oston, 1880: see *Canadian Naturalist*, for Dec. 1880, vol. ix, p. 434.

[39] The fact of the existence at various points in the Appalachian valley of beds of limonite interstratified in Tertiary clays with lignite, as at Brandon, Veront, must not be overlooked. First recognized by Edward Hitchcock, and subsequently noticed by Lesley in 1864, the later observations of Prime, Lewis and others, show the presence of these ores and clays with lignites at various points in Pennsylvania and in Alabama, as well as in Vermont. These are but fragents of what were probably once extended deposits, and although of geological interest as resulting from resolution and re-arrangement in Tertiary time, of a portion of the ancient decayed strata of the valley, are of comparatively little economic importance. (See H. C. Lewis, Proc. Acad. Nat. Sci. Philadelphia, Oct. 7, 1879.

[40] Second Geol. Survey of Penn., Report A, p. 83.

§ 32. Those who have read what I had written on the subject previous to 1876, and especially my discussion of the origin of these ores in 1874,[*] are aware that I have never advocated any such theory. I have, it is true, endeavored to find in the insoluble products of decay of these ancient crystalline rocks, the source not only of the clays and sands of the succeeding sediments, but of their contained iron, whether diffused or accumulated in ore-masses. I have, however, at the same time, always maintained that the ores associated with the so-called Primal and Auroral rocks of the Appalachian basin, like those of the higher horizons, up to the coal-measures inclusive, were deposits contemporaneous with the strata in which the valleys were subsequently excavated; and that save in some cases where, as in the Clinton beds, it was apparently deposited as peroxide, the iron was accumulated in the form of carbonate, and more rarely of sulphid; from the alteration of both of which, *in situ*, the limonites have been formed. This view, which as I then showed, was that advocated by C. U. Shepard, in 1837, for the limonites of western New England, was the same as that put forward in 1838, by R. S. M. Jackson himself, who maintained, as stated in the language of Prof. Lesley, "that the ore belonged to the stratified limestone beds themselves, and had been set free from them by chemical and mechanical decomposition." This history was clear to Dr. Persifor Frazer, who, having remarked that "the theory of alteration *in situ* of various iron-minerals resulting in the formation of many of these limonites, advanced by C. U. Shepard, and ably discussed and adopted by Dr. T. Sterry Hunt, cannot be disregarded in seeking the cause which produced these limonites," adds, "in 1838, and independently of Prof. Shepard's observations, Dr. R. S. M. Jackson reported to Prof. H. D. Rogers, substantially the same conclusion from the study of the limonites of Center and Huntingdon counties."[*]

§ 33. This same view in fact was well stated by Lesley himself in 1864, when he said "the brown-hematite ore-deposits of Mount Alto follow the edge of the slates and sandy limestones," and are "but the residues of these beds after decomposition and dissolution; the honey-combed and altered edges" of the slates and limestones themselves, "after the lime has been washed out of them, and their carbonated and sulphuretted iron has been hydrated and peroxidized; the slates having formed the red and white clays." He further described at one locality of the region in question "an outcrop of almost unchanged blue carbonate of iron and lime, several feet thick."

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[*] [illegible]

[*] Second Geolog. Survey of Penn. Report [illegible]

and evidently in part changing into honey-combed brown hematite ore."[43]

§ 34. In 1867, Mr. Benjamin Smith Lyman, expressed similar views in his account of the limonites of Smyth County, Virginia, found lying below the limestones of No. II (Auroral), where many localities "show the ore unmistakably in regular beds conformable to the other rocks." He at the same time supposed that some of these ore-deposits are, like one noticed in Wythe County, Virginia, due to "the weathering of the upper part of a fissure-vein of iron-pyrites," but maintains that the ores, with such exceptions as this, were "deposited in regular beds of greater or less extent and thickness, at the same time with the other rocks," and from the presence in the limonite of occasional masses of carbonate of iron, concludes that it was originally deposited in this condition."[44]

§ 35. But while it is apparent that the ores in question now found imbedded in clays resulting from the decomposition *in situ* of ancient schists, were previous to that decay enclosed therein as massive siderite, or pyrites, we must not overlook the evidences that in certain cases a process of segregation of diffused iron-oxide has played an important part, alike in ancient and in modern times, in the genesis of limonites. Setting aside, as not relevant to our present inquiry, the formation of bog iron-ores, and ochers, which are directly deposited from ferrous solutions by peroxidation and precipitation, we here recall the contribution to the theory of the origin of imbedded iron-ores made by the late William B. Rogers.

The ferrous carbonate found in the rocks of the coal-measures, has, as he has endeavored to show, been generated from diffused ferric oxide by a process of reduction, carbonation and solution, through waters charged with organic matters from vegetable decay; the carbonate of iron thus formed remaining in some cases diffused through the sediments, and in others becoming concentrated by accretion.[45]

§ 36. This view is to be supplemented by the consideration that carbonated solutions of ferrous oxide formed as above (and often containing organic acids), may, by reacting with beds of carbonate of lime, effect a gradual replacement of the latter by carbonate of iron.[46] The transformation of diffused ferric oxide in sediments into massive limonite imbedded therein, is thus a two-fold process, involving first, the intervention of reducing solutions converting the peroxide into ferrous carbonate, and the concentration of the latter; and second, the change of this latter, through peroxidation and hydratation, into limonite.

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[43] Amer. Philos. Soc. Proc., ix, p. 471-475.

[44] Proc. Amer. Assoc. Adv. Science, 1867, p. 114.

[45] Geological Survey of Penn., 1858, ii. 757.

[46] J. Ville found one liter of carbonated water at the ordinary pressure to hold in solution at 20° C. 1·142 grams of ferrous carbonate, and at 15° C. 1·390 grams. From these solutions neutral alkaline carbonates readily throw down the ferrous carbonate, themselves passing to the state of bicarbonates; and carbonates of lime and magnesia produce the same effect though more slowly. (C. Rendus de l'Acad. des Sciences, Oct., 1881, vol. xciii, p. 443.) The present writer found recently precipitated ferrous carbonate to be temporarily much more soluble, under the

It is evident that the first stage of the process thus indicated by Rogers as taking place in sediments as yet unconsolidated, may also be set up in the disintegrated ferriferous materials resulting from the sub-aërial decay of rocks, and still undisturbed; that is to say, that the infiltration of waters holding dissolved organic matter may give rise in the decomposed mass to concretions of ferrous carbonate, which are subsequently changed into limonite. In this way, a concentration may be effected, through which rocks originally containing a small portion of diffused iron-oxide come to include masses of limonite Illustrations of this process are sometimes seen in the decay of impure limestones or dolomites containing, as is often the case, some ferrous carbonate, in the residuum of which we find the iron accumulated in the shape of crusts or layers of limonite.

§ 37. An instructive example of an analogous process is seen in the limonite which on Staten Island, New York, is found imbedded in a layer of brownish earthy material, sometimes attaining a thickness of twelve feet, which rests immediately upon the serpentine-rock of the region, into which it graduates, and from the sub-aërial decay of which it has evidently been derived, the lower portion of the earthy matrix still preserving the peculiar jointed structure of the underlying serpentine. This decomposed material, though including botryoidal crusts, geodes and concretionary grains of limonite, with occasional druses of chalcedony and of quartz crystals, retains considerable coherence.

The source of this limonite seems to have been the iron-oxide liberated by the decay of the ferriferous serpentine, and the proportion of ore in the superjacent mass shows a direc relation to the color and apparent proportion of iron-silicate ir the serpentine beneath. This limonite, which is now mined t a considerable extent, contains, as several analyses have shown from one to two-hundredths of chromic oxide, which is als known to be present in small amount in the serpentine. A impure argillaceous specimen containing only 59·63 of ferri oxide yielded the writer 2·51 of chromic oxide in a conditio readily soluble in chlorhydric acid.

§ 38. Dr. N. L. Britton, of the School of Mines of Columb

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above conditions, yielding supersaturated solutions, which inclose vessels sp taneously deposit after many hours a large part of the carbonate in a crystalli condition.

College, in whose company I lately had an opportunity of visiting this interesting locality, published in 1880 a geological map with sections and a description of Staten Island.[47] He therein shows that the earthy material in which the limonite is imbedded is confined to the tops of certain hills of serpentine, being absent alike from other similar hills adjacent, and from intervening valleys cut into the serpentine, and he has connected this distribution of the ore-bearing stratum with the facts of the local glaciation of the region, to which he has devoted much attention. It is I think evident that the decay of the serpentine, and the concentration in the residuum, of its iron in the form of limonite, was a process anterior to the glacial erosion, and that the ore-banks are areas of the decayed material which escaped this action.

§ 39. Turning now to the valley of the Mississippi, we find that Pumpelly in his geological survey of Missouri, showed in 1873 that the decay *in situ* of granitic rocks and of quartziferous porphyry has left great rounded blocks of these crystalline rocks, while the conversion of the porphyry into clay, and its subsequent removal, has liberated included veins or masses of crystalline hematite, giving rise to an accumulation of detrital iron-ore, such as, at the well-known Iron Mountain, forms a covering over the surface of the hill of porphyry. From the presence of stratified deposits of this detrital ore in the ancient Cambrian strata around the base of the hill, Pumpelly inferred that the decay of the porphyry was already complete to a considerable depth at this early period.[48] His observations and deductions were not known to me when, in the same year, I published my conclusions as to the great antiquity and the universality of the process of rock-decay.

§ 40. Proceeding from Missouri northward, we find that in Minnesota, as shown by C. A. White[49] in 1870, and by N. H. Winchell in 1874, the ancient granitoid rocks, when protected by Cretaceous strata, support a kaolinized layer of considerable thickness.[50] In Wisconsin, a similar condition of things is found beneath the Potsdam sandstone in the central part of the State, as described by Irving in 1876,[51] in an essay which is a valuable contribution to the literature of kaolin, and contains many analyses of the decayed rocks of the region by Mr. E. T. Sweet, which have been already referred to, § 16. Further details of the same region and its kaolins, with analyses as before, were given by Irving in an essay in 1880 on the

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[47] Annals New York Acad. Sciences, vol. ii, part 6.

[48] Geology of Missouri; Report on Iron Ores and Coal Fields, pp. 8-12.

[49] Geology of Iowa, i, 124.

[50] Second Annual Rep. Geol. Minnesota, pp. 162, 166, 207; also Hunt, Chem. and Geol. Essays, p. 260.

[51] Trans. Wisconsin Academy, etc., iii, 13.

Mineral Resources of Wisconsin."[34] In Jackson and Wood counties, where the crystalline (Laurentian) rocks are covered by a thin sheet of Potsdam sandstone, the river-valleys, cutting through this, expose the kaolin, which "occupies its original position, retaining sometimes the structure of the unaltered rock." This is derived from the decay *in situ* of certain bands, which passing downward, graduate into unaltered feldspathic rock. Save where this mantle of decayed material has been protected by the Paleozoic sandstone, the crystalline rocks are there seen for the most part in an undecayed condition, evidently, as Irving remarks, from the removal of the decayed material by "the denuding action of the drift." In some portions of the driftless area of this region the unprotected gneisses still retain their mantle of kaolinized material.

§ 41. From the facts before us, it is clear that the decay of the Eozoic crystalline rocks was already far advanced in pre-Cambrian times. I am informed that similar evidence is afforded in Sweden by the presence of decomposed rock beneath Cambrian strata. Prof. A. Geikie has moreover shown that the sculpturing of the gneiss rocks of western Scotland, a process which I have maintained to be dependent on previous sub-aërial decay, was effected before the deposition of the Cambrian sandstones, which there rest upon ancient *roches moutonnées.*[35]

§ 42. It might be supposed, from their stability under ordinary atmospheric influences in regions protected by vegetation, that all such portions of decayed Eozoic rocks as still exist in driftless or in protected areas date from the dawn of Paleozoic time, did we not know that the same processes of decay have been active in subsequent ages, as is shown by the decay of eruptive rocks of later periods. An example of this, which shows at the same time the little progress made in the process of decay since the drift-period, is seen in Canada, at Montreal, where, to the south of Mount Royal, the nearly horizontal beds of the impure Trenton limestone are found in sheltered places, deeply decayed and porous from the removal of their carbonate of lime, and are moreover traversed by dykes of dolerite and other feldspathic rocks, themselves decayed to considerable depths; while near by, and especially to the north of the mountain, where glaciation did its work of removing all the decayed aqueous and igneous rocks, the eroded surfaces of both of these are found hard and comparatively unchanged beneath a thin layer of soil and vegetation, as described by J. W. Dawson.

Another instance is afforded by a dyke intersecting the

Potsdam sandstone in this vicinity, which is found to be converted to a depth of twenty feet or more into a plastic highly aluminous clay, which, from the presence of portions of titanium and chromium is, we may conjecture, derived from a doleritic rock.[54]

§ 43. Rigaud Mountain, an igneous mass rising through the Potsdam sandstone, and occupying several square miles on the south side of the Ottawa, near its confluence with the St. Lawrence, is probably of Paleozoic age, and consists in large part of a reddish granitoid orthoclase rock. Considerable areas of its surface, lying lower than the surrounding crests of the mountain, are covered to a depth of seven feet or more, in places, with well rounded bowlders from three to eighteen nches in diameter, consisting wholly of the rock of the mounain, with the exception of a few masses of sandstone. The plains so covered attain in their higher parts an elevation of about 280 feet above the Ottawa, but slope gently both to the outh and the north. The bowlders are very rare on the north slope of the mountain, and at its northern base, but are abundant on the southern slope and in the low-lying clay-covered plains to the southward.[55] These well-rounded masses, spread over so much of the mountain are apparently bowlders of decomposition, still *in situ*, having escaped the denuding agents of the drift-period.

§ 44. Examples of more recent sub-aërial decay of crystalline rocks, under peculiarly favorable conditions, were in 1880 described independently by Jos. LeConte and myself, in the auriferous Pliocene gravel of California. The pebbles of feldspathic and hornblendic rocks occurring in the portions below drainage-level—the so-called *blue gravel*—are unaltered, while above that level the similar pebbles, exposed to the action of meteoric waters, are more or less completely kaolinized, exfoliating, becoming earthy in texture, rusty in color, and in some cases converted into a clayey mass. The pyrites so abundant in the blue gravel, has, in these upper portions, or so-called *red gravel*, been oxidized, and the accompanying lignites have been silicified, and often incrusted with crystallized quartz, from silica liberated in the process of rock-decay through the infiltration of surface-waters.[56]

§ 45. To the porosity of the gravel, and the great amount of surface thus exposed, is to be added the influence of carbonic acid from the decaying lignite, the carbon of which is oxidized as the process of silicification goes on. The amount of carbonic dioxide in the air of certain drift-mines in these auriferous gravels is so great that candles will not burn therein. Mr. D. T. Hughes of San Francisco, a well-known mining engineer, to whose careful scientific observations I have been much indebted, informs me that in the case of a drift-mine 300 feet below the surface, in Table Mountain, Tuolumne Co., Cal., where the foulness of the air was especially remarked, he satisfied himself by appropriate tests of the presence in the air of a large proportion of carbonic-dioxide. If, as there is reason to suppose, the amount of this element in our atmosphere was somewhat greater in former ages than at present, we have in these gravels an illustration of its influence in promoting the decay of silicated rocks. It is not improbable that the sulphuric acid generated by the oxidation of the pyrites present in these gravels may also have aided in the process.

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[54] Report Geol. Survey of Canada, 1878–79, H., p. 7.

[55] Geology of Canada, p. 896.

[56] LeConte, this Journal, xix, 177; Hunt, ibid, xix, 371.

§ 46. The slight evidences of decomposition to be seen in the crystalline rocks of thoroughly glaciated regions, as well as in transported bowlders, makes it probable that the seemingly rapid progress of decay, occasionally observed on exposure, of similar rocks in other regions, sometimes appealed to as evidence of a decomposition now going on, is really but the mechanical disintegration of masses already partially kaolinized in former ages. The crumbling of certain apparently unaltered granitoid rocks, in which the feldspar remains bright and hard, should be distinguished from that which follows chemical decomposition. Such disintegration, due apparently to changes of temperature[57] and the action of frost, is, however, important, and deserves farther study from the fact that materials apparently of similar origin enter into the composition of many derived rocks. Lava-flows are, it has been observed, subject to comparatively rapid sub-aërial decay, but these rock-surfaces differ widely in texture as well as in composition from most crystalline rocks.

§ 47. An essay by Professor Pumpelly on Secular Rock-Disintegration, read before the National Academy of Sciences in April, 1878,[58] is a very valuable contribution to the subject before us. He cites therein my conclusions as to the great antiquity and the universality of the process of rock-decay (to which his own observations in Missouri have contributed important data) and also as to the final removal of decomposed material from northeastern America in the time of the glacial drift. He further notes the little attention hitherto given to the subject of sub-aërial decay, and points out its importance in connection with great problems in dynamical geology. The view that this process of rock-decay is "a necessary preliminary to glacial and erosive action, which removed already softened materials," receives from Professor Pumpelly an extended discussion and application. He proceeds to consider the removal and the re-arrangement of these softened materials by three different agencies. First, the encroachment of the sea upon a subsiding region of decayed rocks; second, the action of land-glaciers, in which he points out that the great mass of disintegrated and water-impregnated rock would become frozen, and included, as it were, in the glacier, sharing in its movements and forming thus a ground-moraine.[59]

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[57] In this connection I venture to recall the attention of geologists to a phenomenon already described both by Dr. Shaler and myself, apparently due to superficial alternations of temperature on certain crystalline rocks, which have resulted in establishing in them, to a considerable depth, a series of rifts or divisional planes parallel to the present surface, which are well known to quarrymen. Instances of this abound; besides those noticed by me in this Journal for July, 1870 (vol. i, p. 89), may be mentioned the gneiss on the opposite slopes of Rolleston Hill, Fitchburg, Mass., and that of Stone Mountain near Atlanta, Ga.; also a remarkable example of comparatively thin horizontal plates at the outcrop of beds of nearly vertical micaceous gneiss in the vicinity of Worcester, Mass.

[58] This Journal, xvii, 133–144.

§ 48. To these modes, with which are to be included the ordinary action of rivers and floods, all acting on the peripheral areas of continents, he adds, for the central areas, removed from these agencies, and rendered desert by geographical conditions, the action of the winds. By these, the decayed rock, according to Pumpelly's extension of the ingenious hypothesis of Richthofen, will be separated into the fine material of the lœss, on the one hand, and the sand and gravel of the desert steppes on the other. He thus explains the condition of the crystalline rocks in northern Asia, from which the decayed mantle has been removed not by glacial but by aërial agencies; while the similar rocks in southern Asia, as in Brazil and the southern United States, are still deeply covered with the products of their own decomposition.

§ 49. Pumpelly further remarks that the surface of the undecayed rock to be laid bare by erosion is necessarily an irregular one, the inequalities depending not upon its original differences in hardness, but upon its resistance to decay under the influence of atmospheric waters. The effect of fractures, joints, veins and dykes in the rock in favoring or retarding the action of this agent would be manifested by still further irregularities of the plane limiting the decomposition of the rock in depth.

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[59] Other agencies than ice may produce a similar displacement of decayed material. Belt, in 1874, in his Naturalist in Nicaragua (page 94), describes a movement of the mantle of decayed crystalline rock on hill-sides in that country, as due to land-slides in wet weather. The layer of ground and "reworked" decayed material resting on the gneiss found by Hartt in Brazil (Scientific Results of a Journey, etc., pp. 28, 573) and referred by him to glaciation, may perhaps be a similar phenomenon. More recently, Kerr, in 1879, has described the results of a slow downward motion of the decomposed surface on mountain-sides in North Carolina, as due to the alternate freezing and thawing of the contained water. To this displaced and modified layer, which resembles that produced by glacial action, he gives the name of *frost-drift*. (Proc. Amer. Inst. M. Engineers, viii, 462.)

Thus the rounded surfaces and the closed rock-basins so often observed in glaciated regions of crystalline rocks are seen to be but the natural results of the process of rock-decay, which preceded and prepared the way for denudation.

§ 50. Similar views as to glacial erosion have since been advocated by Nathorst, and more lately by Reusch, who, in a memoir on the geology of Corsica, presented to the Geological Society of France, in November, 1882,[60] has described the disintegration of the granitic region of Corsica to a depth of several meters, giving to the surface smooth slopes instead of the bold escarpments seen in like rocks in Scandinavia. He notes in these disintegrated rocks in Corsica enclosed balls or ellipsoidal masses, fresh in appearance, but like in composition and in structure to the enclosing rock, which, when detached, have been taken for erratic blocks. With this region, he contrasts the similar rocks near Christiania, in Norway, with hard, rounded surfaces, marked by glacial scratches, where it is difficult to find any trace of superficial decay. He does not believe that the ice of the Glacial period removed any considerable portion of the hard rock, to form fiords, valleys, etc., but supposes "a profound disintegration of the Scandinavian rocks before the Glacial period," and conceives the present relief to "represent the surface of the unaltered syenite after the removal by the glaciers of all the decomposed part."

The salient rock-masses of the Norwegian coast are, like the fiords, arranged in a north and south direction, and this, according to Reusch, corresponds with that of fissures, more or less nearly vertical, which traverse the rock, and, as he well remarks, prepared the way for its disintegration in depth, the extent of which would depend upon differences in the nature of the rock. Many of the lake-basins of this region were, according to him, formed through the removal by glaciers of the decayed material from depressions, while others are due to the action of moraines, serving as dykes.

§ 52. It is difficult to state more clearly the consequences which follow from the conception that the decomposition of rocks is "a necessary preliminary to glacial action and erosion, which removed previously softened materials." Reusch, however, seems, from some misconception, to regard this pre-glacial disintegration of the rocks as distinct from kaolinization, of which the crumbling of the granites in Corsica, as in other regions, doubtless represents an incipient stage, such as we meet with in depth, in regions where the superficial and more completely decayed portions have been removed (§ 46).

[60] Bull. Soc. Géol. de France, xi, 62–67.

CONCLUSIONS.

§ 53. The points insisted upon in this paper may be thus briefly resumed:—

1st. The evidence afforded by recent geological studies in America, and elsewhere, of the universality and antiquity of the sub-aërial decay both of silicated crystalline rocks and of calcareous rocks, and of its great extent in pre-Cambrian times.

2d. The fact that the materials resulting from this decay are preserved *in situ* in regions where they have been protected from denudation by overlying strata alike of Cambrian and of more recent periods; or, in the absence of these coverings, by the position of the decayed materials with reference to denuding agents, as in driftless regions, or in places sheltered from erosion, as in the Appalachian and St. Lawrence valleys.

3d. That this process of decay, though continuous through later geological ages, has, under ordinary conditions, been insignificant in amount since the glacial period, for the reason that the time which has since elapsed is small when compared with previous periods, and also probably on account of changed atmospheric conditions in the later time.

4th. That this process of decay has furnished the materials not only for the clays, sands and iron-oxides from the beginning of Paleozoic time to the present, but also for the corresponding rocks of Eozoic time, which have been formed from the older feldspathic rocks by the partial loss of protoxide-bases. The bases thus separated from crystalline silicated rocks have been the source, directly and indirectly, of all limestones and carbonated rocks, and have, moreover, caused profound secular changes in the constitution of the ocean's waters. The decay of sulphuretted ores in the Eozoic rocks has given rise to oxidized iron-ores, and also to deposits of rich copper ores in various geological horizons.

5th. That the rounded masses of crystalline rock left in the process of decay constitute not only the bowlders of the drift, but, judging from analogy, the similar masses in conglomerates of various ages, going back to Eozoic time; and that not only the forms of these detached masses, but the outlines of eroded regions of crystalline rocks, were determined by the preceding process of sub-aërial decay of these rocks.

ART. XXIV.—*On Mr. Glazebrook's Paper on the Aberration of Concave Gratings;* by H. A. ROWLAND.

IN the June number of the Philosophical Magazine, Mr. R. T. Glazebrook has considered the aberration of the concave grating and arrives at the conclusion that the ones which I have hitherto made are too wide for their radius of curvature. As I had published nothing but a preliminary notice of the grating at that time, Mr. Glazebrook had not then seen my paper on the subject, of which I gave an abstract at the London Physical Society in November last. In this paper I arrive at the conclusion that there is practically no aberration and that in this respect there is nothing further to be desired.

The reason of this discrepancy is not far to seek. Mr. Glazebrook assumes that the spaces are equal on the arc of the circle. But I do not rule them in this manner; but the equal spaces are equal along the *chord* of the arc. Again, the surface is not cylindrical, but spherical.

These two errors entirely destroy the value of the paper as far as my gratings are concerned, for it only applies to a theoretical grating, ruled in an entirely different manner from my own, and on a different form of surface.

I am very much surprised to see the method given near the end of the paper for constructing aplanatic gratings on any surface, for this is the method by which I discovered the concave grating originally, and the figure is the same as I put on the blackboard at the meeting of the Physical Society in November last. I say I am surprised, for Mr. Glazebrook's paper was read at the Physical Society, where I had given the same method a few months before, and yet it passed without comment. Indeed, I have given the same method many times at various scientific societies of my own country. However, as Mr. Glazebrook was not present at the meeting referred to, he is entirely without blame in the matter.

ART. XXV.—*On the Stibnite from Japan;* by EDWARD S. DANA.

THE Yale Museum has recently come into possession of a series of specimens of crystallized stibnite from Japan which are of so remarkable a character as to deserve a detailed description. In the size and beauty of the crystals, and in the great complexity of their form, the Japanese stibnite far outrivals the specimens of the same species from other localities, and takes a first place among metallic minerals. The specimens here described formed part of a lot received by Mr. L. Stadtmüller of this city from a correspondent residing in Japan. Other specimens, also of unusual excellence, were earlier received from Messrs. Ward and Howell of Rochester, which had been obtained by Mr. Ward when in Japan, a year or two since. To what extent specimens from the same source have found their way into European collections, the author is not informed.

The locality, which has furnished these remarkable stibnites, is stated to be Mount Kosang near Seijo, on the island of Jaegimeken Kannaizu (Shikoku) in South Japan. The antimony mines have been worked for a considerable period, and the crystallized specimens are highly valued by the natives as ornaments; Mr. Ward speaks of having observed them mounted in flower pots and used to adorn the dwellings.

The remarkable size of these Japanese specimens is the feature which first attracts the attention. A simple group consists of two prismatic crystals with vertical axes nearly parallel; the longer of the two measures twenty-two inches, and the other fifteen inches in length, the thickness varies from one and one-half to two inches. The prismatic planes are deeply striated; the free extremity in each case is well terminated by brilliantly polished planes. Another very fine specimen consists of half a dozen crystals, grouped closely together in slightly diverging positions so that they are united for a considerable part of their length. The total length of the central crystal of the group is twenty-one inches, and the combined width is six inches; the individual crystals have a width of one and one-quarter to two inches. The prismatic planes in this group are smooth and have the luster of highly polished steel; the crystals are for the most part well terminated. Another isolated crystal has a length of sixteen inches, and there are several finely developed detached crystals ranging from six to nine inches in length. The finest group of crystals has a length of eleven inches and a height of ten and one-half inches. The crystals forming it diverge widely above and below from the central mass, consisting of the same mineral sprinkled over with small quartz crystals, so that they stand free from each other. There are about twenty crystals in the group measuring from three to six inches in length, together with numerous smaller ones. Another group measures eight by nine inches and consists of diverging and interlacing crystals two to six inches long. In other groups the crystals form a comparatively even surface not projecting outward as is more common. A large mass partly massive with imperfect crystallization has a length of sixteen and one-half inches, a height of eight and a width of four inches. In addition to the above there are numerous smaller specimens consisting of groups of highly modified brilliant crystals from half an inch to two or three inches in length.

The specimens in the Yale Museum are especially mentioned here because they include the finest of which the writer has any knowledge. There are, however, in Mr. Stadtmüller's hands numerous other specimens which also deserve mention, being hardly inferior in size or beauty to those which have been described. It would be difficult to name another case on record in which metallic crystallization has taken place on so grand a scale.

The luster of these crystals of stibnite is an important element in their beauty. In all the better specimens this luster is very brilliant, giving the crystalline faces a polish which is rarely excelled. The luster of these natural planes is not less splendent than that of a fresh surface obtained by the perfect cleavage parallel to the brachypinacoid, and can be compared only with that of highly polished steel, as for example the surface of a razor blade. To what extent the brilliancy of the luster will be dimmed by prolonged exposure to daylight can hardly be predicted, but the appearance of older specimens from other localities would make it seem inevitable that this brilliancy should be gradually lost.

From a scientific point of view the complexity of form observed among the Japanese stibnites is their most remarkable character. There have certainly not many cases been observed in which the crystals of a species from a single locality show as many as seventy distinct and well-defined forms.* Previous to 1864 but sixteen planes had been identified. Krenner in his excellent monograph of the species† added twenty-eight new planes, and Seligmann‡ added one more, thus making the list forty-five in all. Of these forty-five planes, thirty have been observed on the Japanese crystals, and in addition to them *forty new planes* have been determined, thus increasing the list to *eighty-five.* This list could be considerably enlarged if the planes admitting of only doubtful determination were to be added. The habit of the majority of the crystals, especially of the larger ones, is tolerably constant. They are always prismatic, elongated in the direction of the vertical axis, and are often more or less flattened parallel to the brachypinacoid; the prismatic planes are generally numerous and often follow each other in oscillatory combinations. The termination of the free extremity is generally formed by the zone between the brachypinacoid $b(010)$ and the unit macrodome $z(101)$, and the three commonest planes are $p(111)$, $\tau(343)$ and $\eta(353)$, of which the plane τ has usually the larger development. Together with these the new plane $\omega_3(5{\cdot}10{\cdot}3)$ is ordinarily present. This occurrence of the zone mentioned containing the pyramidal planes 121, 353, 343, 676, 111, 656, 323, 313, is the common feature of the crystals of the locality. In other crystals the zone between the brachypinacoid (010) and the macrodome Σ(203), containing the pyramidal planes 2·12·3, 283, 273, 263, 253, 243, 233, 223, 213, 629, all but one of which are new, is strongly developed; as many as nine of the planes in this zone have been observed in a single crystal.

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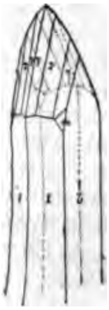
* Bücking enumerates 172 on the epidote from the Untersulzbachthal, Z. Kryst., 321, 1878.

† Ber. Ak. Wien, li, Dec. 9, 1864.

‡ Jahrb. Min., 1880, i, 135.

1.



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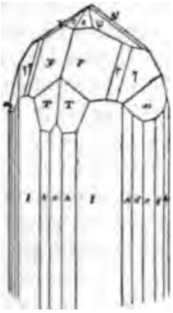

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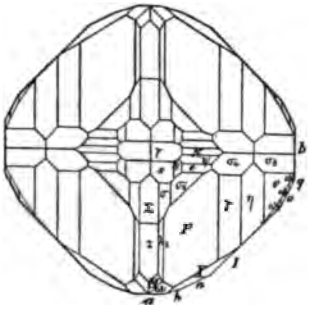

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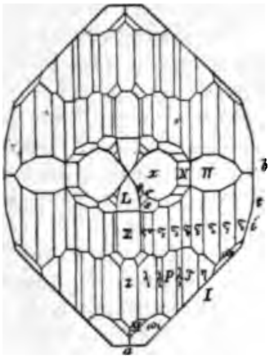

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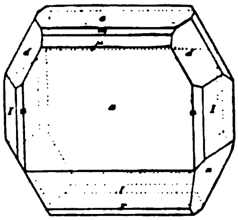

Crystal of "allanite": figure inserted by mistake.

Figures 1 and 2 show common forms, and the projections on the basal planes given in figures 3 and 4 show the development of the two zones mentioned, as also of several other characteristic zones. In some cases the crystals are spear-shaped being sharpened off by very steep pyramidal planes; several of these do not admit of exact determination being rough and rounded, but several of them can be made out, as Π(521), A(361), and so on.

The most complex of the crystals are generally small, varying in thickness from ½ mm. to 1½ mm.; occasional large crystals, however, show a considerable number of planes, these being sometimes so rounded into each other as not to admit of determination. The crystal from which figure 4 was drawn has a length of 7 inches, the planes are mostly sharp and distinct; another crystal, remarkable for its symmetry, but much rounded on the edges, for the opportunity to examine which the writer is indebted to Mr. Clarence S. Bement of Philadelphia, had a length of 7½ inches and a thickness of 1½ (macrodiagonal) and 2 inches (brachydiagonal).

The bending over of the crystals in the direction of the macrodiagonal axis is a very common peculiarity of these crystals, and seems to be characteristic of the species. This has been described at length by Krenner, who mentions crystals so bent as to form a complete ring. With the Japanese crystals this bending is generally confined to the termination, but in some cases it has gone so far that the edge is bent between two adjacent pyramidal planes (*p* or *τ*) so as to make a right angle with itself. Sometimes this results in a simple rounding over of the pyramidal planes, but more commonly they are separated into a series of successive apparent planes. This irregularity is so common that it is not easy to find crystals, even among the smaller individuals, which are quite free from it. Thus in a specimen consisting of a group of small crystals almost all are found to be more or less curved at the extremities. Fortunately this makes itself manifest at once, so that there is no danger of mistaking false planes produced by this irregularity for true crystalline faces. The cause of this common peculiarity it is hard to explain. In the case of some of the larger individuals it would seem that it might have been produced by mechanical means, and, though this is not generally true, in all cases it appears as if produced subsequent to the formation of the crystal. More rarely the slender prismatic crystals have a cork-screw-like twist.

As Krenner (l. c.) has observed, it requires only a slight pressure in the direction of the axis *b* to produce this bending of the crystals. The ease with which it takes place will be appreciated from this circumstance—a slight pressure, accidentally given to a small crystal which was being adjusted for measurement on the goniometer, served to transform it from a symmetrical crystal, capable of giving accurate measurements, into one with curved extremity, quite useless for this purpose. Seligmann (l. c.) has observed that parallel to the base there is often a "gleitfläche" produced by the molecular sliding perpendicular to the brachypinacoid, and Mügge* has more recently called attention to similar phenomena. The natural prismatic crystals are often terminated by a striated surface, having the position of the basal plane, but doubtless to be regarded only as a "gleitfläche."

The exact determination of the axial ratio for the Japanese stibnites was made more difficult in consequence of the irregularity which has just been described. Many crystals of faultless luster, which seemed capable of giving excellent angles, were found to be quite unreliable because of an incipient bending of the character described. In cases where this difficulty did not exist the results were very satisfactory. A number of crystals were subjected to careful measurement and the results obtained from the best one of these deserve to be stated in detail. It showed the planes *p*(111), τ(343), η(353) and ω_s(5·10·3), all giving excellent reflections; there were also present several smaller planes and a number of prisms, but they did not give results accurate enough to serve the purpose in view. As fundamental (supplement) angles were obtained

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* Jahrb. Min., 1883, ii, 19.

$$353 \wedge 3\bar{5}3 = 99° 39' 0'' \text{ and } 353 \wedge \bar{3}53 = 55° 1' 0''$$

The following comparison between the measured angles and those calculated from the above data will show that the angles assumed are as accurate as could be expected, and that the crystal is unusually free from irregularity.

	Measured.	Calculated.		Measured.	Calculated.
$111 \wedge 1\bar{1}1$	70° 48′	70° 48′	$111 \wedge \bar{1}11$	71° 24′	71° 24′
$\bar{1}11 \wedge \bar{1}\bar{1}1$	70° 49′		$1\bar{1}1 \wedge \bar{1}\bar{1}1$	71° 23½′	
$343 \wedge 3\bar{4}3$	86° 55′	86° 55′	$343 \wedge \bar{3}43$	62° 37′	62° 37′
$\bar{3}43 \wedge \bar{3}\bar{4}3$	86° 55′		$3\bar{4}3 \wedge \bar{3}\bar{4}3$	62° 37½′	
$353 \wedge 3\bar{5}3$	99° 39′*	99° 39′	$353 \wedge \bar{3}53$	55° 1′*	55° 1′
$\bar{3}53 \wedge \bar{3}\bar{5}3$	99° 39′		$3\bar{5}3 \wedge \bar{3}\bar{5}3$	55° 1½′	
$5·10·3 \quad 5·\bar{10}·3$	119° 27′	119° 27′	$5·10·3 \wedge \bar{5}·10·3$	51° 34′	51° 34½′
$\bar{5}·10·3 \quad \bar{5}·\bar{10}·3$	119° 27′		$5·\bar{10}·3 \wedge \bar{5}·\bar{10}·3$	51° 34′	
$111 \wedge \bar{1}\bar{1}1$	110° 36′	110° 38′	$353 \wedge \bar{3}\bar{5}3$	126° 26′	126° 28′
$1\bar{1}1 \wedge \bar{1}11$	110° 38′		$3\bar{5}3 \wedge \bar{3}53$	126° 28′	
$343 \wedge \bar{3}\bar{4}3$	119° 5′	119° 5′	$5·10·3 \wedge \bar{5}·\bar{10}·3$	150° 29½′	150° 30′
$3\bar{4}3 \wedge \bar{3}43$	119° 6′		$5·\bar{10}·3 \wedge \bar{5}·10·3$	150° 30′	

The above measurements show that a considerable degree of confidence may be placed in the axial ratio deduced from the fundamental angles selected. This is

$$\breve{a} : \bar{b} : \dot{c} = 1 : 1·00749 : 1·02550.$$

The prismatic angle $I \wedge I$ ($110 \wedge 1\bar{1}0$) = 89° 34′·3.

The axial ratio obtained by Krenner differs somewhat from this, he giving—

$$111 \wedge 1\bar{1}1 = 70° 33'·4 \quad 111 \wedge \bar{1}11 = 71° 39'·6, \text{ and } 110 \wedge 1\bar{1}0 = 89° 5'·8.$$

The following is a list of the planes which have been observed on the Japanese stibnite, those which are new being marked by an asterisk (*). The planes are given in the order of the vertical zones in which they occur:

Pinacoids $i\text{-}\bar{i}$ (100, a), $i\text{-}\breve{i}$ (010, b); prisms $i\text{-}\bar{3}$ (310, h), $i\text{-}\bar{2}$ (210, n), $i\text{-}\bar{\frac{3}{2}}$ (320, ι)*, I (110, m), $i\text{-}\breve{\frac{6}{5}}$ (560, κ)*, $i\text{-}\breve{\frac{4}{3}}$ (340, r), $i\text{-}\breve{\frac{3}{2}}$ (230, d), $i\text{-}\breve{\frac{5}{3}}$ (350, l), $i\text{-}\breve{2}$ (120, o), $i\text{-}\breve{\frac{5}{2}}$ (250, χ)*, $i\text{-}\breve{3}$ (130, q), $i\text{-}\breve{4}$ (140, i), $i\text{-}\breve{5}$ (150, t), $i\text{-}\breve{6}$ (160, ϑ)*, $i\text{-}\breve{7}$ (170, θ)*; macrodomes $\frac{1}{3}\text{-}\bar{i}$ (103, L), $\frac{2}{3}\text{-}\bar{i}$ (203, Σ)*, $1\text{-}\bar{i}$ (101, z), $9\text{-}\bar{i}$ (901, Φ)*; brachydomes $\frac{1}{3}\text{-}\breve{i}$ (013, γ), $\frac{1}{2}\text{-}\breve{i}$ (012, x), $\frac{2}{3}\text{-}\breve{i}$ (023, N), $1\text{-}\breve{i}$ (011, u), $2\text{-}\breve{i}$ (021, Π)*, $4\text{-}\breve{i}$ (041, Y)*; pyramids $\frac{1}{4}$ (114, μ)*, $\frac{2}{7}$ (227, ν)*, $\frac{1}{3}$ (113, s), $\frac{2}{3}$ (223, σ_2)*, 1 (111, p), 3 (331, ξ); macro-pyramids $\frac{8}{9}\text{-}\bar{4}$ (829, Ψ)*, $\frac{4}{3}\text{-}\bar{4}$ (413, M); $\frac{2}{3}\text{-}\bar{3}$ (629, σ_1)*, $1\text{-}\bar{3}$ (313, λ_1)*; $\frac{5}{3}\text{-}\bar{\frac{5}{2}}$ (523, ω_1)*, $5\text{-}\bar{\frac{5}{2}}$ (521, T)*; $\frac{2}{3}\text{-}\bar{2}$ (213, σ); $1\text{-}\bar{\frac{3}{2}}$ (323, λ_2)*, $4\text{-}\bar{\frac{4}{3}}$ (431, X)*; $1\text{-}\bar{\frac{6}{5}}$ (656, λ_3)*; brachypyramids $\frac{10}{3}\text{-}\breve{\frac{10}{9}}$ (9·10·3, Z)*, $\frac{7}{6}\text{-}\breve{\frac{7}{6}}$ (676, β); $\frac{5}{12}\text{-}\breve{\frac{5}{4}}$ (4·5·12, δ)*; $\frac{2}{3}\text{-}\breve{2}$ (246, Γ)*, $\frac{4}{3}\text{-}\breve{\frac{4}{3}}$ (343, τ), $\frac{20}{3}\text{-}\breve{\frac{4}{3}}$ (15·20·3, D)*; $1\text{-}\breve{\frac{3}{2}}$ (233, σ_3)*, $\frac{10}{3}\text{-}\breve{\frac{3}{2}}$ (20·30·9, W)*, $5\text{-}\breve{\frac{3}{2}}$ (10·15·3, E)*, $\frac{8}{3}\text{-}\breve{\frac{8}{5}}$ (583, ω_2)*; $\frac{25}{6}\text{-}\breve{\frac{5}{3}}$ (15·25·6, F)*, $\frac{5}{3}\text{-}\breve{\frac{5}{3}}$ (353, η); $\frac{2}{3}\text{-}\breve{2}$ (123, e), $\frac{4}{3}\text{-}\breve{2}$ (243, σ_4)*, $2\text{-}\breve{2}$ (121, v), $\frac{10}{3}\text{-}\breve{2}$ (5·10·3, ω_3)*, $6\text{-}\breve{2}$ (361, A); $\frac{11}{3}\text{-}\breve{\frac{11}{5}}$ (5·11·3, ω_4)*; $1\text{-}\breve{\frac{5}{2}}$ (255, H)*, $\frac{5}{3}\text{-}\breve{\frac{5}{2}}$ (253, σ_5)*, $2\text{-}\breve{3}$ (263, σ_6)*; $\frac{10}{3}\text{-}\breve{3}$ (10·30·9, V)*, $\frac{7}{3}\text{-}\breve{\frac{7}{2}}$ (273, σ_7)*; $\frac{2}{3}\text{-}\breve{4}$ (146, ψ), $1\text{-}\breve{4}$ (144, G)*, $\frac{8}{3}\text{-}\breve{4}$ (283, σ_8)*, $4\text{-}\breve{6}$ (2·12·3, σ_9)*.

The above list includes 70 planes, of which 40 are new to the species. In addition to the above there have been observed 15 additional planes, as given below; of these the basal plane, given by Haüy, must be considered as doubtful; Hessenberg gives the planes w and ρ, Seligmann observed g, and the remainder are given by Krenner.

O (001, c)?; $i\text{-}\bar{\frac{4}{3}}$ (430, k); $\frac{1}{6}\text{-}\bar{i}$ (106, R), $\frac{1}{2}\text{-}\bar{i}$ (102, y); $\frac{4}{3}\text{-}\breve{i}$ (043, Q), $\frac{5}{3}\text{-}\breve{i}$ (053, J), $3\text{-}\breve{i}$ (031, j), $\frac{9}{2}\text{-}\breve{i}$ (092, g); $\frac{1}{2}$ (112, π); $\frac{1}{2}\text{-}\bar{2}$ (214, f); $1\text{-}\bar{\frac{4}{3}}$ (434, a); $1\text{-}\bar{\frac{8}{7}}$ (878, ϵ), $3\text{-}\breve{3}$ (131, w); $\frac{4}{3}\text{-}\breve{4}$ (143, ϕ); $\frac{5}{3}\text{-}\breve{5}$ (153, ρ).

In conclusion, I state here briefly the observations upon which the determinations of the new planes are based. The measured angles, though sufficient to determine the planes, could seldom be exactly obtained. In the *prismatic zone* the new planes were determined by angles measured on the brachypinacoid b (010), as follows: for ι (320) 56° 50′, required 56° 30½′: for κ (560) 40° 4′, required 40° 1′: for χ (250) 22° 9′, required 21° 57′: for ϑ (160) 9° 42′, required 9° 32′; for θ (170) 8° 21′, required 8° 11½′. The new *macrodomes* were determined by angles measured on the macropinacoid a (100), as follows: for Σ (203) 55° 40′, required 55° 38½′; for Φ (901) 6° 20′, required 6° 11′. The new *brachydomes* were determined by angles measured on the brachypinacoid b (010), as follows: for Π(021) 26° 20′, required 26° 9¾′: for Y(041) 14°, required 13° 48′. In the zone of the unit pyramids three new planes were observed: μ (114) determined by the angle on I (110) $=70° 17′$, required 70° 8′: ν (227) also in the zone with x (012) and Σ (203); and σ_2 (223) also in the zone with 203 and 010.

In the zone between 203 and 010 as many as 10 pyramidal planes were observed, all but one on a single crystal, and of these 9 are new. The measured angles on Σ (203) were for σ_1 (629) 10° 30′, for σ_2 (223) 29° 30′, for σ_3 (233) 40°, for σ_4

(243) 48° 1′, for σ_5 (253) 54° 17′, for σ_6 (263) 59° 7′, for σ_7 (273) 62° 55′, for σ_8 (283) 65° 10′. The corresponding calculated angles are respectively σ_1 10° 34½′, σ_2 29° 15½′, σ_3 40° 2½′, σ_4 48° 15′, σ_5 54° 28′, σ_6 59° 16′, σ_7 62° 58½′, σ_8 65° 57′. In addition, the plane σ_9 (2·12·3) was determined on another crystal, being in a horizontal zone with Y (041), and the angle 9° for 2·12·3 on 041 being given, calculated 9° 16′.

In the zone between z (101) and b (010) in which 8 planes have been previously determined, three new planes were observed; they were fixed by the angles on the brachypinacoid, viz: for λ_1 (313) 77°, required 76° 40½′; for λ_2 (323) 64° 30′, required 64° 39′; for λ_3 (656) 59° 30′, required 59° 22′. The common plane ω_3 (5·10·3) was determined by the measurements given in the table above; in the same horizontal zone (503 to 010) with it, three other planes were observed, viz: ω_1 (523) also in the zone I (110) to z (101), $\omega_1 \wedge I = 36°\ 30'$, required 36° 6½′; also ω_2 (583) determined by the angle on b (010) 35° 31′, required 35° 0½′; ω_4 (5·11·3) which was also in the zone q (130) and η (353), the measured angle on b (010) was 27° 59′, required 27° 57′.

In the zone with a (100) and ω_3 (5·10·3), two planes were determined, viz: V (10·30·9) measured angle on $\omega_3 = 8°$, required 7° 56′; and W (20·30·9) measured angle on $\omega_3 = 7°$, required 7° 0′. In the zone with I (110), ω_3 (5·10·3) and σ_7 (273), three additional planes were determined, viz: D (15·20·3) measured angle on I (110)=10°, required 10° 32′; E (10·15·3) measured on I(110) 14° 30′, required 14° 35½′; F(15·25·6) measured on I (110) 19°, required 18° 2′. The measured angle of I (110) on ω_3 (5·10·3) was 23° 29½′, calculated 23° 29′. Of the remaining planes X (431) was in the zone I (110) to z (101) and the measured angle on I was 14°, required 13° 42′. The plane I (346) was in the zone N (023), ψ (146) σ_2 (223), the angle measured on N was 23° 20′, required 22° 58½′. The plane Ψ (829) was in the zone σ_1 (629) and a (100), the following angles were also measured $\Psi \wedge \sigma$ (203)=12° 30′, required 12° 23′; $\Psi \wedge 213 = 10°$, required 9° 56′. The plane T(521) was determined by the following measurements: $T \wedge T$ (521 ∧ 5$\bar{2}$1)=42° 15′, required 42° 14′; $T \wedge h$ (310) 11°, required 10° 48′; $T \wedge I = 31°\ 45'$, required 32° 16′. The planes G (144) and H (255) were in the zone with u (011) and a (100), the angles measured on u were 10° and 15° 30′ required 10° 11′ and 16° 2′.

Since the above pages were written the writer has received from Mr. Stadtmüller, to whom he is otherwise much indebted, some additional material which promises to afford some new points; the study of this is necessarily deferred.

August 3, 1883.

ART. XXVI.—*Notes on the Volcanoes of Northern California, Oregon and Washington Territory;* by ARNOLD HAGUE and JOSEPH P. IDDINGS, of the U. S. Geological Survey.

DURING the autumn of 1870 the geologists attached to the Geological Exploration of the Fortieth Parallel made a preliminary reconnaissance of several of the extinct volcanic cones of Northern California, Oregon and Washington Territory for the purpose of planning detailed investigations of the principal volcanoes of the Sierra and Cascade Ranges. A further study of these volcanoes however was never undertaken, and although the explorers brought back most interesting geological and lithological material the results and observations of the work have never been published, with the exception of an announcement in this Journal for March, 1871, of "The discovery of actual glaciers on the mountains of the Pacific Slope," and a popular paper on "The volcanoes of the United States Pacific Coast," read by Mr. S. F. Emmons before the American Geographical Society, March 13th, 1877.

Among the more prominent peaks along this belt of volcanic cones may be mentioned Lassen's Peak and Mount Shasta in California; Mount Pitt, Three Sisters, Mount Jefferson and Mount Hood in Oregon; and Mounts St. Helens, Adams, Rainier and Baker in Washington Territory. From this long line of volcanoes the geologists of the Fortieth Parallel Survey selected for the purpose of exploration the four peaks which, from their size, position and geological relations, might be taken as typical of the chain. Mr. Clarence King explored the two great cones of California; at the same time, Mr. S. F. Emmons undertook the examination of Mount Rainier, while Mt. Hood was visited by one of the writers of the present article. The collections which they made at that time were deposited in the cabinet of the survey.

Lassen's Peak is situated in California just north of the fortieth parallel of latitude, where the continuity of the bold crest of the Sierra Nevada Range breaks down and is replaced by lower and less regular ridges. From Lassen's Peak this magnificent chain of volcanoes extends northward at irregular intervals for nearly five hundred miles. The principal volcanoes follow in general the axial lines of the Sierra and Cascade Ranges, breaking out either along the main line of upheaval or at short distances to the westward. Volcanic extrusions along fissure lines and flows of lava of greater or less extent unite the main peaks, forming a nearly continuous belt of igneous rocks.

Mount Rainier is the grandest of all the volcanoes of the Northwest and forms the most prominent topographical object in Washington Territory, rising proudly above all other peaks, and towering far above the crest of the Cascade Range which lies about twenty miles to the eastward. The surface features of the western portion of the territory have been greatly modified by the great lava-flows of the volcano, and no less than four important rivers of the territory rise among the glaciers of the mountain: the Nisqually, Puyallup and White Rivers, which flow into Puget Sound, and the Cowlitz which, running in a southwesterly direction pours into the Columbia. Snow and ice cover the top of the volcano, reaching downward for five or six thousand feet, while with the most marked contrast the broad base of the mountain supports a dark, dense forest vegetation of great grandeur. The summit of Mount Rainier is formed by three peaks, the highest situated to the eastward of the other two and separated from them by deep and nearly inaccessible gorges, although they attain within a few hundred feet of the same altitude. The main peak presents a perfect, circular cone with a crater about a quarter of a mile in diameter. The altitude of the peak as determined by the United States Coast Survey is 14,444 feet.

Mount Hood is situated directly on the crest of the Cascade Range, about twenty-five miles south of the Columbia River. It has an altitude of 11,225 feet above sea-level, rising about 6000 feet above the main range, but the long lava flows extending in every direction from the base of the volcano have buried beneath their mass nearly all remnants of preëxisting rocks. Mount Adams and Mount St. Helens, on the north side of the Columbia River, form with Mount Hood a triangle, the area of which has been the center of great volcanic activity. The summit of Mount Hood is a single peak, a portion of a rim of an ancient crater, opening to the south. The crater is about one-half a mile wide from east to west, the encircling wall for three-fifths of the circumference rising 450 feet above the snow and ice which fills the basin.

None of the volcanoes along this belt occupy so comparatively isolated a position as Mount Shasta, which stands upon an open plain with the neighboring hills and ridges many thousand feet lower. The great range of the Sierra has fallen away and is followed by low obscure ridges, confused and broken up by volcanic outbursts. The altitude of Mount Shasta is given at 14,440 feet above sea-level, and as the neighboring ridges rarely attain an altitude of over 3000 feet, the volcano presents an imposing spectacle, surpassed by few mountains in the world. As seen from the west the volcano presents a double cone, with the smaller built upon the flanks of the larger one and about 2000 feet lower. The main cone possesses a very regular outline, with remarkably steep slopes of uniform angle. Around the broad base of Mount Shasta numerous lesser cones have broken out, offering a great variety of volcanic phenomena. One of these, which has been named Little Shasta, rises to more than 3000 feet above the neighboring valley. Seventy miles southeast of Mount Shasta, not far from the boundary between Nevada and California, occurs Lassen's Peak, lying, as already mentioned, along the direct line of the Sierras, where the granite of the main range has been abruptly broken down. The region has been the seat of great volcanic activity lasting through long periods of time, the present mountain forming but a remnant of former extrusions, and so far as geological interest is concerned is probably unsurpassed by any other mountain. Lassen's Peak, however, is by no means as conspicuous an object as many of the volcanoes, being surrounded by other cones of considerable elevation, all of them rising out of a great volcanic table. The altitude of Lassen's Peak is given at about 10,500 feet. It is a broad, irregularly shaped mountain with four prominent summits and abundant evidences on the slopes of comparatively recent extrusions of lava.

As the rocks brought back may be considered as representing the principal types of the ejected lavas from the different flows, a large number of thin sections have recently been prepared for the purpose of comparative study with the volcanic rocks of the Great Basin, and their microscopic examination has been followed up by chemical investigation. While a cursory examination of these rocks shows certain special characteristics in color, habit and form of crystals which in many cases easily identifies hand specimens with one or the other of the volcanoes, a comparative study of their mineralogical, structural and chemical features shows the closest identity in the nature of the ejected material from the four volcanoes. In their lithological characters they possess so many features in common that a general description might be given which would be applicable to them all.

These four great cones, which may be taken as typical of the chain, are all andesite volcanoes, with extrusions of basalt breaking out upon their slopes and along the edges of the plain extending in all directions for long distances. From the compact basic basalts of normal type to the more porous acidic andesites similar variations in mineral composition and minute details of structure are found at each of the volcanoes. This similarity holds good not only in macroscopic characters, but is still more marked in microscopic structure. In size, color and distribution of porphyritic crystals, in structure of groundmass, ıature of the glass base, and in the transitions from a pure glass to a crystalline condition, many rocks from cano may be easily correlated with those from one or the other peaks.

similarity in chemical composition is strikingly shown following analyses of typical rocks from each of the four es. Nos. I, II, III, are taken from the report of the cal Exploration of the 40th Parallel. No. IV, as well y all the rest of the chemical work referred to in this vas done by Mr. P. W. Shimer under the supervision of ɔmas M. Drown, in his laboratory at Easton.

TABLE I.

	I. Mt. Rainier.	II. Mt. Hood.	III. Mt. Shasta.	IV. Lassen's Peak.
SiO_2	61·62	63·28	60·44	62·94
Al_2O_3	16·86	17·96	18·12	18·14
Fe_2O_3		1·18		
FeO	6·61	3·16	5·16	3.82
CaO	6·57	5·34	6·43	6·28
MgO	2·17	2·50	3·43	3·06
Na_2O	3·93	3·81	4·09	3·83
K_2O	1·66	2·06	1·25	1·22
Ignition		0·12	0·89	0·60
				TiO_2 0·41
				P_2O_5 0·10
	99·42	100·04	99·79	100·40

e the rocks from these volcanoes in general present the resemblances, there is a wider range and a greater of structure in the more acidic types from Lassen's nd Mount Shasta. On the other hand, judging from ections, the range in the character of the extrusions is stricted at Mount Rainier.

hese rocks then may be classified under the following

lt, composed of plagioclase, augite and olivine, as essen- ıerals.

rsthene-Andesite, having plagioclase, hypersthene and ıs essential minerals.

blende-Andesite, with plagioclase, hornblende and pyrox- essential minerals.

te, composed of plagioclase, mica, hornblende and quartz ıtial minerals.

lt is represented by but few hand specimens which in general to the type found throughout the great basin, they are more or less dense rocks, sometimes quite ar, varying in color from dark to light gray, and occa- mottled. They show no porphyritic crystals other than olivine, and those only in small crystals. In thin sectio they appear to vary in degree of crystallization. The mor coarsely crystalline have very uniform grain, and are composed of lath-shaped plagioclase feldspars with high extinction angles; irregular grains of light brownish green augite, and colorless olivine sometimes in porphyritic crystals with magnetite in minute crystals and particles aggregated in irregular patches, accompanied by a little glass that fills the interstices between the other minerals. The smaller grained varieties have a more porphyritic structure,' and consist of a groundmass of lath-shaped plagioclase crystals, with augite and magnetite and glass base in varying proportions, through this are scattered small porphyritic plagioclases and olivine and occasionally augite.

Hypersthene-andesite.—The hand specimens of this rock are in general very porous, with rough surface and hackley fracture. They vary in color from blue-black to steel-gray, with red and reddish gray varieties. They also occur in all stages from thoroughly crystalline dense forms to almost white glassy pumice. For the most part they are crowded with very small porphyritic crystals of which the feldspars are the most noticeable, the Fe, Mg, silicates being more apparent in the lighter colored varieties. The thin sections show a striking uniformity in the nature and character of the porphyritic crystals, but great variation in the structure and condition of the ground mass, which affords a most interesting field for studying the gradations between a brown glass base and a holocrystalline groundmass. The porphyritic feldspars in the thin sections appear to be wholly plagioclase, no orthoclase having been recognized, they are very fresh and present between crossed Nicols a most beautiful zonal structure, and quite irregular bands of twining, which with sharply defined straight edges vary in length, breadth and numbers, and are formed both after the albite and pericline laws. Inclusions of glass are very abundant in some crystals and entirely wanting in others. When of comparatively large size they are scattered irregularly through the crystal, but when very minute they occur in aggregations that sometimes form zones at the margin or center of the feldspar. From the size of the symmetrical extinction angles observed it would appear that a part of the plagioclase is anorthite, but that for the most part the feldspar is labraborite or andesine, and such was found to be the case when the feldspars from the hypersthene pumice were analyzed chemically. (See analyses III and IV, table II.)

The Fe, Mg, silicate of this rock is pyroxene, partly hypersthene, partly augite, the hypersthene predominating. In thin section the latter is a light brown mineral with a tinge of green, which in polarized light shows a strong pleochroism, be-ng green when the axis *c* is parallel to the principal plane of the polarizer and yellowish brown at right angles to it, of different intensities parallel to the *a* and *b* axes. It occurs in well developed crystals and in fragment, and sections parallel to *c* are often quite free from cleavage lines. Cross-sections show the pinacoids more strongly developed than the prism faces, and the prismatic cleavage more distinct than the pinacoidal. It frequently bears numerous glass inclusions and magnetite grains. It is very easily confounded in thin sections with augite in ordinary light, which with very similar outline is of a pale green color; but the two are more readily distinguished between crossed Nicols where the hypersthene shows orthorhombic extinction and less brilliant interference colors than the augite which besides has in general more or less inclined extinction. In order to demonstrate beyond all doubt by both chemical and optical tests that the strongly pleochroic pyroxene observed in so many thin sections was really orthorhombic and hypersthene, as had been supposed, it was desirable to isolate the mineral from all other constituents and examine it in a pure state. The rock selected for the purpose came from a long slope of Mt. Shasta designated on the label as a mud flow. It is a porous straw-colored pumice with a comparatively simple mineral composition and was chosen on account of the large amount of orthorhombic pyroxene which it contained and the ease with which it could probably be isolated. The pumice was largely composed of nearly colorless glass through which were scattered minute crystals of pyroxene and feldspar which under the microscope proved to be triclinic; there was a little apatite present, but neither hornblende nor mica could be detected. As the very friable pumice could easily be crushed between the fingers, although a very tedious operation, the greater part of the pyroxene was separated from the rest of the rock constituents without injury to the terminal faces of the minute crystals. The remainder of the pyroxene was then separated from the greater part of the feldspar and glass by means of the Thoulet solution of iodide of mercury in iodide of potassium. Under the microscope the isolated pyroxene was seen to consist of about 99 per cent of the strongly pleochroic brown mineral, the remainder consisting of light green augite. Adhering to the pyroxene were minute fragments of glass and a little feldspar which could not be removed without crushing the crystals. The separated mineral was then passed under the microscope and the augite carefully removed by hand. In order to free the mineral from glass and feldspar it was treated as described by M. Fouqué in his admirable work, "Santorin et ses Éruptions," with concentrated fluohydric acid, which completely decomposed the impurities. The white deposit of silica, owing to its low specific gravity, was easily removed by water and mechanical methods. This isolated pyroxene was found to consist of dark brown crystals in stout prisms bounded by two pinacoids, a prism and pyramid, and when rotated about the *c* axis on a needle point between crossed Nicols they showed the extinctions in the prism zone always parallel to that axis. Examined in convergent light the interference figure was that of a biaxial mineral with large angle between the optical axes and with a negative bisectrix perpendicular to the plane of the pinacoid, $(? \infty P \bar{\infty})$, the plane of the optical axes being parallel to the other pinacoid. It was therefore proved to be an orthorhombic mineral with negative optical character. The green augite which was separated from this had a slight pleochroism, was very free from inclusions, and when rotated between crossed Nicols showed its monoclinic character, the maximum extinction angles being 42° in the plane of the clinopinacoid. The interference figure seen in a section parallel to the orthopinacoid was a bar located a little to one side of the field of the microscope, showing an optical axis nearly at right angles to this pinacoid, the plane of the optical axes being parallel to the other pinacoid.

In the second column of the subjoined table is given an analysis of the orthorhombic pyroxene, which shows that the material analyzed was remarkably pure hypersthene without admixture of augite, the impurities being such as are derived from foreign inclusions within the crystals. These results show the mineral from the Mount Shasta pumice to agree more closely with the theoretical composition of hypersthene than that from Buffalo Peak in Colorado, as given by Whitman Cross,* or with that from Santorin as published by M. Fouqué.†

To the feldspar, hypersthene and augite of first consolidation in these hypersthene andesites must be added the largest grains of magnetite scattered through the rock. As remarked before, the character of these porphyritic minerals remains quite constant while the structure of the groundmass presents every possible degree of consolidation, from pure glass to a completely crystallized aggregation of magnetite, pyroxene, feldspar and quartz. In general the groundmass consists of more or less glass base, sometimes globulitic, at others pure, full of lath-shaped and rectangular feldspars, smaller pyroxene crystals and magnetite grains, which vary greatly in size in different specimens. The darker colored rocks have a groundmass rich in brown or globulitic glass with small microlites of feldspar and pyroxene; the lighter colored ones having colorless glass with much larger microlites of feldspar and pyroxene and magnetite. The minuteness and intimate association of the minerals of such a groundmass renders a mechanical separation of them for chemical analysis impossible, and interferes also with a perfect isolation of the larger feldspars. But in the case of the pumice from which the hypersthene just described was extracted the groundmass is an almost pure glass, and as the microscope shows that the hypersthene was the first of the essential minerals to crystallize from the original magma, an analysis of the rock is of considerable interest. In the first column of table II is given a complete chemical analysis of the pumice which does not vary materially from the analyses of the more compact lavas given in table I. Lithologically it differs from the other rocks in being more porous and lighter in color as the iron is nearly all taken up by the hypersthene and a trace of magnetite. The phosphoric acid is accounted for by the presence of apatite. The titanic acid is probably associated with the magnetite. As shown by the analyses the trace of manganese is all concentrated in the hypersthene as well as the greater part of the iron and magnesia. After separating the heavier minerals from the feldspar and glass, the latter were subjected to the Thoulet solution in order to completely isolate the glass from feldspar. An amount of glass was finally obtained which gave optical tests of carrying very little if any feldspar. It was however a much more difficult task to free the feldspar from particles of adhering glass, and only after repeated trials was a product secured which contained a nearly pure feldspar. All attempts to determine two distinct feldspars by differences in specific gravity proved futile and it seems probable that the greater number of them belonged to one species. In volcanic rocks rich in glass base, containing minute feldspars it would seem nearly impossible to completely separate such species as labradorite and oligoclase. The difficulty is not so much owing to slight differences in specific gravity as to the impurities in the crystals, so that a labradorite rich in glass inclusions might readily be of less specific gravity than a pure oligoclase. Two separations of feldspathic material were obtained; one with specific gravity from 2·66 to 2·68 and the other from 2·64 to 2·66, analyses of which are given in columns III and IV. The two products are strikingly similar in composition and do not agree with any known species. In all probability they represent the same feldspar rendered more or less impure by foreign ingredients. Nevertheless the results are exceedingly interesting, showing so far as they go that the heavier portion is richer in lime and magnesia and carries somewhat more iron, while the lighter portion holds more silica and alkalies. As the original rock carries considerable lime, of which very little is taken up by the pyroxene, it is reasonable to suppose that all the larger feldspars are labradorite mingled with others which may be andesine and oligoclase. An analysis of the glass which forms the base of the pumice is given under number V. It is highly siliceous, showing a falling off in alumina, the feldspar-making element which together with the lime had not entered into the first crystallizing mineral, hypersthene. On the other hand the analysis shows a considerable relative increase of alkalies with the greater part of the potassa of the original rock, which had not been taken up by the minerals which crystallized out concentrated here in the residual glass.

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* Bulletin of the United States Geological Survey. No. 1, 1883.

† Santorin et ses Éruptions, Paris. 1879.

TABLE II.

	Pumice. I.	Hypersthene. II.	Feldspar. III.	Feldspar. IV.	Glass base. V.
SiO_2	62·00	50·33	56·41	56·95	69·94
Al_2O_3	17·84	0·97	27·39	27·47	15·63
FeO	4·40	22·00	0·69	*trace*	1·89
CaO	5·37	1·88	9·87	9·10	2·49
MgO	2·64	23·29	0·09	0·02	0·28
K_2O	1·47	....	0·36	0·48	2·85
Na_2O	4·29	....	5·43	5·78	3·83
MnO	*trace*	0·64	....	....	....
TiO_2	0·17	....	....	....	....
P_2O_5	0·29	....	....	....	....
Ignition	1·66	....	....	....	3·25
	100·13	99·11	100·24	99·80	100·16

Specific gravity of III = 2·66 to 2·68. IV — 2·64 to 2·66. V = 2·29

Hornblende-Andesite.—The examples of hornblende-andesite are of two kinds; one which resembles the hypersthene-andesite very closely and is distinguished from it solely by the abundance of hornblende in porphyritic crystals; another that has a compact gray groundmass with large, brilliant, black hornblendes and has the same appearance as the celebrated domite from the Puy de Dôme in Southern France. The first mentioned variety is best described by saying that it has the same general habitus and microstructure as the lighter colored kinds of hypersthene-andesite and similar mineral constitution with the addition of a reddish-brown hornblende, in some specimens brownish-green, which in places has a small black border. This type of rock then is composed of plagioclase feldspar, hornblende, hypersthene, a little augite and magnetite, that are found in porphyritic crystals embedded in a groundmass of the same, generally with the exception of hornblende, with or without glass. The feldspars appear to be less basic than those in the hypersthene andesite, giving extinction angles corresponding to andesine. The second variety of hornblende andesite, which is represented only by hand specimens from Strawberry Valley near Mount Shasta, when in thin sections is seen to consist chiefly of a groundmass of lath-shaped feldspars, smaller pyroxene micro-crystals and a little magnetite with or without colorless glass, through which are scattered a few small porphyritic plagioclases and relatively very large hornblendes, which in some specimens are reddish brown, in others brownish green. They are occasionally surrounded by a border of magnetite and augite grains, which in some cases have entirely replaced the hornblende substance. There are no porphyritic pyroxene crystals; of the porphyritic feldspars, a few appear to be as basic as anorthite, a very interesting fact when the silica percentage of the rock, 65·27 per cent, is considered, and which may account for the small amount of pyroxene in the ground mass and for its being all hypersthene. A chemical analysis of the rock would probably show a relatively large amount of alumina.

Dacite.—A fourth type of rock occurs as an extreme volcanic outburst about four miles to the northwest of the highest point of Lassen's Peak, which for several reasons requires a detailed notice. Owing to the broken and rough nature of the place, the appropriate name of Chaos has been applied to the region. All geologists who have visited the place agree that it is one of the latest, if not the most recent extrusion from the peak, as is shown not only in the hot springs and in the constant escape of steam and gases, but in its relation to the surrounding rocks. The rock is not only quite different in mineral composition and structure from anything else in the collection from Lassen's Peak, but is quite unlike the rocks brought in from the other volcanoes. Baron von Richthofen, who visited the volcano during his studies upon the relative age of volcanic rocks, which he afterwards published in his very able work, "A Natural system of Volcanic Rocks," regarded this rock as one of great interest, and mentions the locality as a typical one for a variety of rhyolite which he named nevadite, or granitic rhyolite from its close resemblance to granite. He says, "geologically nevadite appears to have been produced as frequently by volcanic activity as by massive eruption. An interesting occurrence is that at Lassen's Peak, where it was first discovered by Prof. W. H. Brewer and Mr. Clarence King." A few years later Mr. King revisited the place and brought back the specimens upon which the present observations are based. Our investigations lead us to quite different results, and we can not regard the rock as in any sense entitled to be classed as a rhyolite, as we fail to find among the many sections of feldspar any determinable sanidine, an essential constituent of rhyolite. Upon a superficial examination the hand specimens bear a singularly close resemblance to granite, especially to the more common types of the Sierra Nevada, but a closer scrutiny brings out a fundamental difference, as the rock is rich in glass base, through which the secreted minerals are scattered. The specimens vary from a moderately compact rock to others of a pumice-like character, the glass being drawn out in fine hairs or threads across the cavities and open spaces, giving it a porous appearance. It is a quartz-mica-hornblende-plagioclase rock and may be classed as a dacite, the quartz-bearing member of the andesite group, and it is the only rock in the collection in which quartz is so abundant as to be regarded as an essential constituent. All the thin sections of this rock examined under the microscope showed the closest identity in structure—a colorless glass full of gas cavities of all sizes, through which are found relatively few microscopic crystals of plagioclase, and hornblende with less mica, pyroxene, magnetite, apatite and zircon. This forms a porous ground mass which bears large porphyritic crystals of plagioclase, mica, hornblende and quartz, and very rarely pyroxene. Nine-tenths of the feldspars showed lines of striation, the remainder appearing to be brachypinacoidal sections of the same species, showing strong zonal structure. No sanidine was detected in any of the sections. The feldspars contain very large and abundant glass inclusions, some with as many as nine gas bubbles in one globule of glass. The mica is reddish brown in basal sections, with small angle between the optical axes. The hornblende is greenish and reddish brown and is without dark border. The pyroxene is both hypersthene and augite. Quartz that appears so abundantly in the hand specimens is seldom seen in thin section, as it is in cracked grains that drop out in grinding; when met with it is seen to contain only glass inclusions in dihexahedral forms. The following chemical analysis of this dacite, given in column I, table III, shows it to be from 6 to 9 per cent richer in silica than the prevailing rocks of the four main cones.

TABLE III.

	I.	II.	III.
SiO_2	69·36	65·77	76·75
Al_2O_3	16·23	21·51	12·32
Fe_2O_3	0·88		
FeO	1·53	trace	1·36
CaO	3·17	5·72	1·18
MgO	1·34	0·00	0·00
Na_2O	4·06	5·92	3·55
K_2O	3·02	0·83	3·98
Ignition	0·45	0·34	0·54
	100·04	100·09	99·68

The analysis itself would indicate that the rock belonged to the dacites, for in addition to the increase in silica there is an increase in the percentage of alkalies, principally of potassa, with a corresponding decrease of iron, lime and magnesia. If on the other hand we compare this analysis with the published and unpublished analyses of rhyolites from the Great Basin we find a relatively low percentage of alkalies and too large amount of lime, magnesia and iron. In other words it has the chemical composition of a dacite. To determine more accurately the composition of the feldspathic material, the feldspar and glass were separated from the heavier minerals by the Thoulet solution, and the products subjected several times to the same treatment until the separation was nearly complete. The result gave a glass nearly free from feldspar, and a feldspar with some glass particles, but regarded as not enough to vitiate the value of the analysis. In column II of the third table will be found the feldspar, and in column III the residual glass. The analyses show clearly that there could be but little if any sanidine present. The feldspar analyzed, although a little high in silica owing to the glass as an impurity, agrees closely with the analyses usually given for andesine. It may be a mixture of andesine and oligoclase. It will be seen that by far the greater part of the potassa of the rock remains concentrated in the glass, as is the case with the hypersthene-andesite pumice. In this case the potassa in the residual glass is in excess of the soda, while in the crystallized feldspar it bears to soda the proportion of about 1 : 7. To confirm if possible the results obtained by microscopic and chemical investigation upon the feldspars in the dacite, we picked out a number of crystals to test by Dr. Szabó's admirable and delicate method of determining feldspars in rocks by means of the color imparted under certain conditions to the flame of a Bunsen burner. For the application of this method we are greatly indebted to Dr. Szabó, who during his visit to this country a year ago kindly gave us practical instruction to enable us to carry out his tests. All feldspars examined gave the reactions for andesine or oligoclase, completely confirming the results obtained by other methods.

The four types of rocks just described, namely: basalt, hypersthene-andesite, hornblende-andesite and dacite exhibit, it is true, four well characterized groups, but they cannot be said to be sharply defined and distinct forms. Indeed between any two in the order named exist all possible intermediate varieties. Thus among porphyritic basalts are found, those with diminishing olivine and increasing hypersthene which may equally well be called hypersthene-bearing basalts or olivine-bearing hypersthene-andesites. And right here it may be well to point out the relationship existing between hypersthene and olivine. A microscopic study of these intermediate rocks in conjunction with chemical determinations has shown that hypersthene is the substitute for olivine in rocks differing from basalt by a somewhat higher silica percentage, the former mineral being, as it is, simply the bi-silicate of the same Fe, Mg, elements. It is not strange then to find rocks of similar structure and appearance differing solely in the relative proportion of olivine and hypersthene, and passing by easy gradations from basalt to hypersthene-andesite.

Between hornblende-andesite and hypersthene-andesite occur varieties in which only a small amount of hornblende is found, and which with equal propriety might as hand specimens be considered as modifications of one or the other types.

In the same way forms are found intermediate between hornblende-andesite and dacite, whose proper connection only field observation can determine. It will be remarked that no mention has been made of augite-andesite; among the andesites in this collection from the volcanoes none have been found in which the predominating pyroxene is augite.

The foregoing general descriptions apply to the varieties of lava from whichever of the four great volcanoes they are found, and it only remains to point out what the varieties are that occur at each volcano as learned from the present collection. Mount Rainier is formed almost wholly of *hypersthene-andesite*, with different conditions of groundmass and accompanied by hornblende and olivine in places. The rocks from Mount Hood are olivine-bearing *hypersthene-andesite* approaching basalt: hornblende-bearing *hypersthene andesites* and *hornblende-andesites*. From Mount Shasta were collected *basalts* both dense and porous and of varying shades of color; *hypersthene-andesites* in great variety, from compact to pumiceous and of many colors, some bearing more or less hornblende and presenting the transition forms to hornblende-andesite, which occurs in abundance. The variety resembling domite is from Sisson's Cone and Black Butte, Strawberry Valley near Mt. Shasta.

From Lassen's Peak have been brought in *basalt*, *hypersthene-andesite* with small amount of hornblende; *hornblende-andesite and dacite*. One variety of the hornblende-andesite has very small porphyritic crystals. A chemical analysis being given in table I, column IV. Another variety especially from what have been called the earlier and later flows, has quite large crystals of glassy-looking plagioclase, hornblende and some mica and less quartz. The rock from the later flow has fewer porphyritic crystals than that from the earlier one, and is accompanied by a variety of hornblende-andesite of most curious structure. It is a very porous evengrained mass composed of tabular plagioclases, hornblende and colorless glass.

the hornblende playing the same rôle as the augite of many diabases, its irregular form filling the spaces between well-developed plagioclase crystals, which in part appear to be anorthite and have crystallized before the hornblende. There is a little pyroxene present and considerable tridymite.

ART. XXVII.—*Cassiterite, Spodumene and Beryl in the Black Hills, Dakota;* by WILLIAM P. BLAKE.

CASSITERITE is found in place and in stream deposits in the central region of the Black Hills, Dakota, about two miles from Harney in Pennington County. This tin ore occurs in a vein, or mass, of coarsely crystalline granite rising through the fine-grained micaceous and siliceous slates of the Huronian period flanking the granitic axis of the Harney range of mountains.

At the principal point of discovery, known as the Etta claim, the feldspar quartz and mica are in unusually large crystals. Some of the cleavage blocks of feldspar measure from twelve to twenty inches across and the mica crystals have yielded sheets of commercial value. In addition to these minerals there is an abundance of spodumene in gigantic crystals ranging from two to six feet in length and from eight to twenty inches in diameter. The first masses of tin ore found were imbedded in the midst of the crystals of spodumene and of feldspar, and yielded fragments of nearly pure cassiterite weighing from one to fifty pounds and more. The chief associate or gangue of the tin ore is, however, a dense aggregation of small crystals of mica, of a light yellow color, forming irregular veins and bunches in the midst of the granitic mass. In this micaceous rock, formed of the mica with a white spar apparently albite, the tin ore is disseminated in grains from the size of a mustard seed to the size of peas, and sometimes an inch in diameter. These grains are irregular in form; no perfect crystals have yet been found. They can be easily separated from the mica by crushing and washing. The ore appears to be very clean; no wolfram has yet been found with it in the mica, but a few crystals of a black mineral, believed to be wolframite, have been seen in the mixture of spodumene and feldspar. No topaz has yet been identified.

At another locality where large plates of muscovite-mica have been mined, I have found beryls in large crystals, imbedded in a matrix of quartz, resembling the beryls found in the mica mines of Acworth, New Hampshire, though not so large.

ART. XXVIII.—*Discovery of a new Planetoid;* by C. H. F. PETERS. (Communication to the Editors, dated Litchfield Observatory of Hamilton College, Clinton, N. Y., Aug. 15, 1883.)

I TAKE pleasure in sending notice of a new planetoid that I fell upon in the night of August 12th. It is bright 9th magnitude. The following two observations are considered good, the positions of the comparison stars being well ascertained. The filar micrometer, bright wires in dark field, with a power of 270, was used.

1883.	m. t.		
	h. m. s.	h. m. s.	
Aug. 12.	13 49 28	α [234]=21 20 48·17	δ [234]= −12° 29′ 8·9″
14.	12 46 11	α [234]=21 19 40·44	δ [234]= −13 7 33·0

From the very strong motion in declination we may infer that the planet is comparatively near to the earth.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

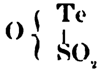
1. *On the Hypothesis of Prout.*—GERBER has suggested a modification of the hypothesis of Prout, based upon the dynamic assumption that the atom is the result of an ether undulation. A primary undulation in this medium would produce secondary waves having relations with it analogous to those of sound vibrations. Representing one of the elements of the undulation, the velocity for example, by the atomic weight, the author has sought for harmonic relations between the atomic weights of different elements and has found that they may be divided into four classes, distinguished by the dominant valence of the elements which they contain, and each of which has a common divisor, in general greater than unity; so that the atomic weights of bodies of the same class are exact multiples of this common divisor by simple factors. For the first division or class the factor is 0·7673 or 0·76923, according as O is assumed as 15·96 or 16. For the second division 1·995 or 2. For the third division 1·5586 or 1·5625. And for the fourth division 1·246875 or 1·250. The relation between these factors and hydrogen—first pointed out to the author by Pliny Earle Chase—is given as follows: for O=15·96, $\alpha_1=\frac{10}{13}H=0·7673$, $\alpha_2=H$ or $2H=1·9950$, $\alpha_3=\frac{25}{16}H$ or $(\frac{5}{4})^2H=1·5586$, and $\alpha_4=\frac{5}{4}H=1·246875$. For O=16, the values are, of α_1

76923, of α_2 2·000 or 1·000, of α_3 1·5625 and of α_4 1·2500. ables are given showing the accord between the calculated and e experimental atomic weights. The author concludes that ere exists: (1) a common unit, in general greater than 1, beween all the elements which belong to the same natural group; 2) simple ratios between the four common units, the multiples of hich include all the atomic weights with an approximation as lose as can be desired; and (3) simple relations between the eight of all the elements, distinct from those claimed by Prout. -*Bull. Soc. Ch.*, II, xxxix, 562, June, 1883. G. F. B.

2. *On a modified form of V. Meyer's Vapor density apparatus.* -SCHWARZ has substituted the ordinary combustion furnace for e special furnace employed by V. Meyer in determining the apor density of substances of high boiling point. A trough of eet iron a millimeter thick, having a close-fitting cover, is aced in the furnace. Two sloping sheets of iron cover the rnace, in place of the ordinary clay pieces. The trough has pports for the tube, and is lined with asbestus. This tube is osed at one end and has a capacity of 170–180 c.c. From its pen end a rubber tube communicates with the collecting tube. he substance is introduced in a porcelain or platinum boat, or liquid, in a bulb with a fine open point. In making a determiation the tube is placed in the furnace with about 10 cm. proecting from the end, a cork ring preventing its being pushed ack. A double screen of sheet iron is placed between the cork nd the furnace. The tube is filled with dry nitrogen, the eighed substance is introduced at the cool end, the connections re made with the collecting tube, the furnace is tipped back so s to allow the boat to slide down the vaporizing tube, and is ested on a block. Soon bubbles of air enter the collecting tube nd the process terminates in a minute or less. The water is eveled, the volume of air read, the thermometer and barometer oted and the vapor density calculated by V. Meyer's formula. umerous examples of results obtained in this way are given. nthracene 88·6, cal. 89·0; anthraquinone 105·1, cal. 104·0; amonium chloride 13·67, cal. 13·375; triphenyl-methane 125·7, cal. 22·0, etc. The author has shown that soft paraffin has always a wer vapor density than hard; the former gave 57·2 correspondg to the formula C_8H_{18}; the latter 123·2, the formula $C_{17}H_{36}$ quiring 120.—*Ber. Berl. Chem. Ges.*, xvi, 1051, May, 1883.

G. F. B.

3. *On a new Tellurium oxide, and on a new Reaction of Telrium.*—DIVERS and SHIMOSÉ have obtained a new oxide of telrium by heating in a vacuum the compound of sulphur trioxide d tellurium until it decomposes. It is a solid body which on ating decomposes into tellurium dioxide and free tellurium, the tter volatilizing before the former. The sulphoxide was heated a tube retort in a paraffin bath, the tube being connected with Sprengel pump. It fuses to a pasty mass, then decomposes, tumescing very much and evolving sulphur dioxide; its brown

color turns to the black of the new oxide which now resembles charred cork. The vesicular mass is rubbed to powder and washed with water containing a little sodium carbonate to remove sulphur compounds and tellurous acid; then with pure water and alcohol and dried. It is stable in ordinary dry air, is black in color with a brown shade, has a graphitic luster when pressed, has the formula TeO or a multiple of it, is decomposed by potassium hydrate on boiling, and by hydrochloric and sulphuric acids in the cold, is oxidized readily by nitric acid and colors sulphuric acid red as it dissolves in it, tellurous sulphate being deposited from the solution. It appears to have neither acidic nor basic properties. The authors have also prepared and critically examined tellurium sulphoxide, obtained by acting on tellurium with sulphur trioxide in a closed and exhausted tube. Heat is evolved and the loose gray tellurium passes into the bulky deep red sulphoxide. After digestion at 30°–40° the excess of SO_3 was drained off, the last portions being removed by exhaustion with a Sprengel pump the tube being heated to 35°. Thus prepared, tellurium sulphoxide is an amorphous solid, which softens at about 30° without melting; of a beautiful red color transparent in thin layers, quite stable when kept in closed tubes; having the formula SO_3Te; and decomposed by water into tellurium, tellurium monoxide, tellurous acid, sulphurous and sulphuric acid. It appears to exist in two modifications, the red variety being converted instantaneously into a brown substance at 90° which has identically the same composition. To the red modification the authors assign the formula  $O\left\{\begin{matrix}Te\\ |\\ SO_2\end{matrix}\right.$; to the brown variety the formula $O\left\{\begin{matrix}TeO\\ |\\ SO\end{matrix}\right.$.

In a subsequent paper, Divers and Shimosé have described a new and delicate reaction for tellurium. When sulphuric acid holding in solution a minute quantity of tellurium dioxide is poured into a hydrogen apparatus, the escaping gas will contain hydrogen telluride. If now this gas is passed into the telluretted sulphuric acid the red color of tellurium sulphoxide is developed in the liquid. If the gaseous current be continued too long, the red sulphoxide is destroyed and a brown insoluble substance appears, sometimes in black scales with metallic luster, which is probably hydrogen pertelluride.—*J. Chem. Soc.*, xliii, 319, 323, 329, June, 1883. G. F. B.

4. *On the Production of Sulphides by Pressure.*—SPRING has continued his researches upon the production of chemical compounds by means of pressure and has now given the results of exposing various metals mixed with sulphur, both finely divided, to a pressure of 6500 atmospheres. Magnesium after six pressings, each time being reduced to filings, gave a homogeneous gray mass having a weak metallic luster. Mixed with water at 50° or 60°, hydrogen sulphide was evolved, the water becoming gold

yellow and giving a precipitate of sulphur on the addition of a drop of acid. Zinc after three pressings gave a sulphide resembling the natural blende, which dissolved slowly in dilute sulphuric acid evolving hydrogen sulphide. Iron gave after four pressings a block which was scarcely touched by the file and which appeared homogeneous under the microscope. It evolved steadily H_2S with sulphuric acid. When heated in a tube no further reaction took place, the mass slowly melting. Cadmium sulphide was formed easily in three pressings, the mass being homogeneous and yellowish gray. Bismuth sulphide was formed in two pressings, lead sulphide still more easily. Silver required six to eight pressings before the mass became homogeneous. Copper yielded the sulphide in three pressings and so also did tin. Antimony gave a grayish black sulphide in two pressings. It had the luster of antimony glance and dissolved in hot hydrogen chloride with the evolution of H_2S. Aluminum gave an imperfect result, as also did carbon. Both this latter body and red phosphorus formed hard blocks when pressed with sulphur which appeared like compounds but in which no phosphorus sulphide or carbon sulphide could be detected. From these results Spring draws the conclusion that allotropic states are but different conditions of polymerization, the chemical activity decreasing as the process goes on.—*Ber. Berl. Chem. Ges.*, xvi, 999, May, 1883.

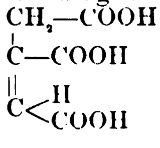
G. F. B.

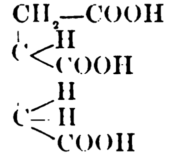
5. *On Double Orthophosphates of Barium.*—Barium hydrate, as is well known, is quite soluble in concentrated solutions of the alkali silicates. DeSchulten has utilized this fact in the preparation of the double orthophosphates of barium, with sodium and with potassium. On adding a solution of potassium phosphate to a concentrated solution of potassium silicate containing barium hydrate, no change is at first observed; but on agitating strongly the liquid becomes milky and deposits microscopic cubes. To obtain larger crystals, the silicate solution is heated to boiling and a second silicate solution, containing the potassium phosphate, is added. On cooling, limpid cubical crystals are deposited. After rapid washing in cold water and drying, they dissolve easily in dilute hydrogen chloride, leaving a residue of silica amounting to about one per cent. Considering this as an impurity, analysis gives the formula $KBaPO_4, (H_2O)_{10}$ for this double phosphate. The crystals effloresce slightly in the air and do not act upon polarized light. By substituting sodium salts for those of potassium, sodio-barium orthophosphate is obtained in regular tetrahedrons, which are efflorescent and contain about two per cent of silica.—*Bull. Soc. Ch.*, II, xxxix, 500, May, 1883. G. F. B.

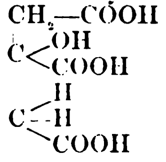
6. *On Saponin.*—Saponin was discovered in 1809 by Schrader in the root of *Saponaria rubra.* Stütz has submitted it to a thorough investigation in Geuther's laboratory. To prepare it ten kilograms of the bark of *Quillaja saponaria* was extracted with water, the liquid evaporated to a thick syrup, dried upon porcelain plates, pulverized, extracted with boiling alcohol, the

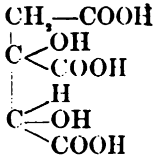
extract allowed to cool, and the separated flocks filtered off and redissolved in hot alcohol and again allowed to separate on cooling; the process being repeated till the deposited saponin was perfectly white. The yield was 200 grams. As thus prepared, saponin is a white amorphous powder, neutral in reaction, and when absolutely pure, tasteless. It is soluble in every proportion in water, insoluble in absolute alcohol and ether. The most dilute aqueous solutions froth like solutions of soap. It contains 2·4 per cent of ash, chiefly potassium, calcium and magnesium carbonates. Analysis led to the formula $C_{19}H_{30}O_{10}$. By adding barium hydrate to its aqueous solution, a precipitate was obtained having the formula $(C_{19}H_{30}O_{10})_2$, $Ba(OH)_2$. By treatment with acetic oxide, the saponin was acetylized giving either $C_{19}H_{25}(C_2H_3O)_5O_{10}$ or $C_{19}H_{25}(C_2H_3O)_5O_4(OC_2H_3O)_4$, according to the time of the action. From these reactions the author concludes: (1) that the formula of saponin is $C_{19}H_{30}O_{10}$; (1) that five of the oxygen atoms are in the form of hydroxyl and two in that of combined oxygen doubly united to the carbon. Of the nature of the other three no conclusion can be drawn. Hence the formula may be written $C_{19}H_{25}(OH)_5O_2.O_3$; (3) that by the action of the acetyl oxide, the five hydroxyls are first replaced by acetyl and subsequently the two oxygen atoms successively, are made linking and unite the acetyls to the carbon, as in the case of aldehyde and ethylene oxide.—*Liebig's Ann.*, ccxviii, 231, May, 1883. G. F. B.

7. *On a New organic acid in the Juice of the Beet Root.*—Von Lippmann has examined the incrustations formed upon the pans in which beet juice is evaporated. Beside citric, aconitic, tricarballylic, and malonic acids, he has now isolated a new acid, which was obtained by fractional solution in ether and evaporation. The resulting syrup after standing two years became a mass of needle-shaped crystals, soluble in water, alcohol and ether and having the formula $C_6H_8O_8$. The barium salt is $(C_6H_5O\;)_2$ Ba_3, $(H_2O)_6$, the acid being tribasic. This acid appears to be identical with the oxycitric acid described by Pawolleck, prepared from chlorcitric acid, itself obtained from aconitic acid by union with hypochlorous acid; though Pawolleck could not obtain his acid crystallized and says his lime salt contains nine instead of ten molecules of crystal water. The author gives the following rational formulas to these closely allied acids:


Aconitic acid.
$C_6H_6O_6$


Tricarballylic acid.
$C_6H_8O_6$


Citric acid.
$C_6H_8O_7$


Oxycitric acid.
$C_6H_8O_8$

—*Ber. Berl. Chem. Ges.*, xvi, 1078, May, 1883. G. F. B.

8. *On the Silicic Ethers of the Phenols.*—MARTINI and WEBER have called attention to the facility with which the silicic ethers of the phenols may be prepared by heating silicon tetrachloride with on excess of the phenol. In this way they have obtained tetraphenyl orthosilicate $Si(OC_6H_5)_4$ and tetra-*p*-cresyl orthosilicate $Si(OC_7H_7)_4$. Both of these bodies may be distilled unchanged. The phenyl compound is syrupy when cold, and becomes slowly crystalline. The paracresyl ether is solid from the first and may be obtained beautifully crystallized. The authors propose to continue their researches upon these ethers.—*Ber. Berl. Chem. Ges.*, xvi, 1252, June, 1883. G. F. B.

II. GEOLOGY AND MINERALOGY.

1. *Report of an exploration of parts of Wyoming, Idaho and Montana, in August and September*, 1882, *made by Lieut.-Gen.* P. H. SHERIDAN, *with the Itinerary of Col.* J. F. GREGORY, *and a Geological and Botanical Report by Surgeon* W. H. FORWOOD.—In the geological report, Surgeon Forwood gives the results of observations showing that changes in the geyser action of the Yellowstone Park are in progress, and new geysers making at times their appearance. The following is from pp. 47, 48.

"The very careful observations of Dr. Peale in 1872 on the frequency, height, duration, etc., of the principal geysers in the upper basin, show no perceptible diminution of their power, as compared with the performances of these geysers at our visits in 1881 and 1882. The Castle, Bee-Hive, Old Faithful, Saw-Mill and Turban Geysers appear to perform just as they did ten years ago. The Grotto Geyser may have increased a little in frequency and diminished in duration, and the Grand and Giantess have changed somewhat in character and for the better, as will be seen by the following notes.

Grand Geyser.—Dr. Peale records the following observations in 1872:

August 18.—One continuous eruption, lasting 15 minutes.

August 19.—One continuous eruption, lasting 37 minutes.

August 20.—A succession of three eruptions, with intervals as follows: First eruption, three minutes; interval, six minutes. Second eruption, four minutes; interval, ten minutes. Third eruption, nine minutes. Total time, thirty-two.

Messrs. E. S. Dana and Grinnell (Ludlow's report) observed, August, 1875, a succession of *five* eruptions, with short intervals.

August 25, 1881.—There was a succession of *seven* eruptions, with short intervals, one to three minutes; total time estimated, thirty to forty minutes; and on the same day, two hours later, the same performance was repeated.

August 26.—A succession of seven eruptions, as before; not repeated that day.

August 19, 1882.—(Reported by eye-witnesses.) Geyser played at 2 P. M., and again an hour later, giving a fine display of seven eruptions each time.

August 20.—A succession of seven eruptions, with intervals substantially same as last year, but not repeated.

August 21.—Same as yesterday.

It appears, then, that in 1872, the geyser often discharged at a single eruption, with occasional successions of as many as three. In 1875 there were a succession of five eruptions, and in 1881 and 1882 there were never less than seven observed, and these were not unfrequently repeated an hour or two later.

Giantess Geyser.—In 1872, as recorded by Dr. Peale (Hayden's report, p. 149), there were three eruptions of about seventeen minutes each, at intervals of three-quarters of an hour.

In 1875, as observed by Dana and Grinnell, the performance was as follows: After some preliminary efforts, during which a large amount of water was thrown out, there was an interval of two hours, when a similar disturbance took place, and two hours later the geyser played to a great height for about an hour, after which there was a violent escape of steam for an hour or more longer.

In 1882, as observed by Mr. John Baronett, the geyser bega[illegible] August 6, 8 P. M., and played twenty feet or more for about fi[illegible]teen minutes; then followed an interval of about the same tim[illegible] then it played twenty to fifty feet, with occasional sprays up [illegible] 150 feet for three-quarters of an hour, followed by an interval about the same time. Continued to play with intermissions same way up to midnight, and was still playing in morning a[illegible] up to 12 M. August 7, when it ceased, and the crater was emp[illegible] to a great depth.

August 13, 5.30 P. M., it began with a rumbling noise and sh[illegible] up 'higher than Old Faithful,' and then varied between 20 a[illegible] 150 feet for three-quarters of an hour, and an intermission about the same time followed. Thus the eruptions and suspe[illegible]sions, about three-quarters of an hour each, continued throu[illegible]out the 13th, 14th, and up to 9 o'clock on the morning of the 15[illegible] when action ceased and the crater was empty as before.

A new geyser of the first class appeared about three years a[illegible] in place of some hot springs, a short distance west of the Gia[illegible] called the Splendid, and well deserves the title. It has three j[illegible] two directed obliquely inward and one vertical in the center, a[illegible] plays to a height of fifty to seventy-five feet once in about t[illegible] and one-half hours.

The Sheridan Geyser is another new one, situated on the F[illegible] Hole River, about half way between the Upper and Lo[illegible] basins, and is the largest in the park. It was one of a grou[illegible]p hot springs designated by Dr. Peale as the 'Half-way Spring[illegible]' 1872 (see Hayden's Report, 1872, p. 147). It began to pla[illegible]y a irregular intervals, and, finally, within the last year, settled d[illegible]wn to a constant period of about two hours. It was impossibl[illegible]e at our hasty visit to obtain measurements or even correct estim[illegible]ates of the dimensions of the crater, owing to the dense clouds of steam that constantly envelop it from sight, but while in eruption the column appeared to be about 200 feet in diameter and rose to a great height, accompanied by vibrations of the surrounding surface and the throwing out of rocks, which cover the ground and the bed of the river for a hundred yards on all sides. Its character as an eruptive geyser was first pointed out by the distinguished mountaineer, Mr. John Baronett, who named it in honor of General Sheridan in 1881."

2. *Report on the Thermal Springs of the Yellowstone National Park;* by A. C. PEALE. From the 12th Annual Report (for the year 1878) of the U. S. Geological and Geographical Survey under Dr. F. V. HAYDEN, U. S. Geologist-in-Charge. 454 pp. 8vo, with numerous maps and plates.—This Report is based mainly on observations in the Park made in the year 1878. But it includes also a review of the facts connected with other thermal springs in America, and with those of foreign countries. The descriptions of the springs and geysers of the Park make Part I of the volume; and they are accompanied by many illustrations which add greatly to the interest and value of the Report. Part II—covering pp. 304–354—treats briefly of the same phenomena in other countries and other parts of the United States, and Part III discusses the general subject of Thermo-hydrology, and consists largely of facts and views presented by other authors, but with a reference to the bearings on these views of the facts from the Yellowstone Park. This valuable Report closes with an extended bibliography, or list of papers on the Park and on American and foreign thermal waters.

The following notes are cited from its pages. The total number of springs in the Park is stated to be about 3000, and the number of ascertained geysers 62, besides 9 that are mentioned with a query. The following throw up the water to a height of 200 feet, or beyond: the Giant, 200; Castle, 200; Grand, 200; Giantess, 250, and Beehive, 230. The jet of Old Faithful is stated at 150 feet.

Bunsen's theory of the action of geysers is accepted (after a consideration of other views), as in the main sufficient. But there are differences in action among them which require special explanation. For example, in Old Faithful and Beehive, the action is simple, it consisting of a water eruption in successive jets that follow each other rapidly, without a succeeding period of steam emission; whereas in Castle Geyser, the type of another class, the water eruption is followed by a steam period of considerable length. In still others, under each of these divisions, the water period is broken into a number of water ejections, with varying intervals of minutes or hours, while the regular interval in the water periods is a day to several days.

The amount of silica found on analysis by Leffmann, in waters from the Yellowstone Geyser basins, varied from 53·76 grains in an Imperial gallon out of 143·22 grains of solid contents, to 3·36 grains out of 83·30 of solid contents; the former at the Opal spring of the Gibbon Basin, the latter at the Mammoth Hot Springs. The water from the Opal spring, on concentration to a small bulk over a water bath, affords a gelatinous deposit which finally dries to a mass of pulverulent geyserite. Next to silica the most abundant material is sodium chloride (common salt) which at the Opal spring amounted to 82·18 grains in the gallon, while calcium salts are often wholly absent or only in traces. In most of the springs a deposition of silica does not take place on cooling, evaporation being necessary.

Besides the ordinary geyserite, which contains usually 9 to 13 per cent of water, (and the Pealite in which the water is 1 to 6 per cent), an unstable variety which, when still moist, makes an incrustation looking a little leathery, but dries to a soft, easily crumbling mass, is named, by E. Goldschmidt, *Viandite*. The "steam-dry" material was found to afford about 75 per cent of water to 20 of silica; but the describer says that "very probably the compound possesses no stability."

3. *On the cause of the Glacial Period;* by SEARLES V. WOOD (Geol. Mag., x, July, 1883, pp. 293–302).—The general conclusion of this paper is given on page 150. The following is a notice of the author's course of argument prepared for this Journal by Mr. W. J. McGee.

In this memoir, which embodies the cosmologic ideas developed during his extended researches in the Newer Pliocene of England, Mr. Wood reiterates and accepts as fatal to the eccentricity theory the correct arguments against repeated alternation of cold and warm intervals during the Quaternary, and assails Tyndall's dictum that general terrestrial refrigeration could not inaugurate glacial conditions. Also, following Dana in America, he shows that the traces of Quaternary ice-extension sustain a definite relation to present precipitation and temperature distribution; from which he infers, *first*, that the assumption of a polar ice-cap is an error, *second*, that all theories involving considerable geographical changes, alterations in the position of the terrestrial axis, or variations in the obliquity of the ecliptic, are alike invalid, and *third* (since the Gulf of Mexico in its present condition is the chief vapor-source of eastern North America), that Croll's position in regard to the Quaternary diversion of the Gulf Stream feeders is untenable. The prevailing theories thus disposed of, he points out that while a diminution of temperature will materially reduce the rainfall, it may not reduce, but may even increase the snowfall and annual ice-accumulation, and cites the present considerable ice-accumulation in Greenland, despite the low precipitation there, in proof of the soundness of his view; from which he concludes that the Quaternary ice was probably developed under lower temperatures than those now obtaining. In explanation of the assumed cooling of the earth during the Glacial period, he adopts the chemical theory of solar energy as advocated by Siemens and others, assumes that the heat-supplying material is irregularly diffused through the space traversed by the solar system, and supposes that solar emission is hence variable.

4. *Deep-sea Magnesian Limestone Nodules.*—Prof. Verrill, on page 447 of the last volume of this Journal, describes nodules or masses of limestone from a few inches to nearly thirty in length, dredged up on the Gulf Stream in depths of 234 to 640 fathoms. Science of August 10th gives an analysis of one—a hard, compact kind, sp. gr.=2·73—by Prof. O. D. Allen, as follows: lime 24·95 per cent, magnesia 14·41, iron (estimated as protoxide) 2·00, insoluble residue (sand) 16·97, phosphoric acid not weighed. It contained minute specks of pyrite.

III. BOTANY AND ZOOLOGY.

1. *Genera Plantarum ad Exemplaria imprimis in herbariis Kewensibus servata definita.* Auctoribus G. BENTHAM et J. D. HOOKER. Londini: Reeve & Co. 3 vols. imp. 8vo. 1852–1883. —The completion of this great work marks an era in systematic botany. As the senior author is nearly eighty-three years of age, we may say that, essentially, it brings to a worthy close the long course of scientific labors of the most accomplished, sagacious, and continuously industrious phytologist of our day, almost the only survivor of those who personally knew Antoine Laurent Jussieu, and were associated with the elder De Candolle; one whose line of authorship, begun in the year 1826 (antedated only by that of the venerable Roeper), comes down to the present year, apparently in almost unabated vigor, and is as remarkable for sustained importance as it is unparalleled in length. Upon the junior author, who should not yet feel the burden of years, however weighted with official cares, and upon such specialists as can be mustered, may devolve the responsibility of such *addimenta* as may be needed to bring the earlier volumes up to the date of the last, and even to supplement the work from time to time, as the "Genera Plantarum" of Linnæus was supplemented by successive editors. But it is not probable that the task which has now been happily accomplished will be undertaken again, still less executed, before the twentieth century is well entered upon. Useful compilations we may expect, and monographs which may here and there better represent advancing knowledge of particular groups; but a production like this, covering the whole field of phænogamous botany, ordinally and generically, and with the uniformity of treatment and scale thus secured by two close associates in one continuous work, cannot be looked for again for a long while.

It has few predecessors. The first "Genera Plantarum," in fact (though not exactly in name), was that of Tournefort, published in the year 1700. Then and there, as Linnæus declared, genera of plants, in the sense of scientific botany, were first established. There would probably be more recognition of this *dictum* if the present work were to be planned anew, and the genera which Linnæus himself admitted as of Tournefort, along with those which modern botanists have restored, could have been attributed to this real founder, without thereby compromising the proper position to hold in respect to herbalistic and ancient names. The second "Genera Plantarum" was that of Linnæus, in 1737, of which the last edition revised by the author himself was that of 1767. The third was that of Jussieu, "secundum ordines naturales disposita," which appeared in the year 1789. That of Endlicher—a monument of literary or bibliographical erudition rather than of botanical research—was brought out in the main between 1836 and 1843, at about the same time with the more unpretending synoptical compilation of Meisner. These were important in their way. But the "Genera Plantarum" of Bentham and Hooker, which began to be issued in the year 1862 and was finished in the spring of the present year, is the lineal successor of the three classical works above mentioned, that of Tournefort representing the botany of the close of the seventeenth century; that of Linnæus the first half, and that of Jussieu the latter part, of the eighteenth century. The present work—increased from the one small octavo of Linnæus to three thick imperial octavo volumes of nearly 1,200 pages each—stands in like relation to the nineteenth century, and is based, like them, or even more than they, upon actual investigation, and upon the comparison of a vastly greater number of original types than was formerly possible.

Unlike its predecessors, however—and in this respect agreeing with the other great botanical work of the century, the "Prodromus" of De Candolle—the whole of cryptogamic botany is omitted. This vast field must be left to specialists. Fries, of Upsala, who died a few years ago, at a ripe old age, was the last phænogamic botanist who was at the same time master of one or two cryptogamic orders; and now even the best of cryptogamists can hardly aspire to more than a general and superficial acquaintance with any other department than the one to which he devotes himself. This inevitable state of things has its disadvantages. The reasons for it do not really apply to the ferns and their allies, and it was naturally expected, as it is much to be desired, that these should enter into the present work. May we hope that this still may be?

Some idea of the progressive enlargement of the field may be had by a comparison of the number of genera characterized in these successive works. The phænogamous genera of

Linnæus,	"Gen. Pl.," ed. 1,	A. D. 1737,	were		887
"	" ed. 6,	A. D. 1764,	"		1,189
Jussieu,	"	A. D. 1789,	"		1,707
Endlicher,	"	A. D. 1843,	"	(about)	6,400
Bentham & Hooker,	"	A. D. 1883,	"		7,585

If the last had been elaborated upon the scale of Endlicher, or with the idea of genera which is still common if not prevalent, the number of genera would have amounted to at least ten thousand. An estimate of the number of known species of each genus and higher group has been made throughout the work—a rough approximation only, mentioning first the number in the books, and the number to which, in the opinion of the authors, these may probably be reduced by botanists who adhere to the Linnæan view of species; from which it appears that upon the very strictest estimate their number, as now known to botanists, is at least 95,620. In round numbers, it may fairly be said that about 100,000 species of phænogamous plants are in the hands of botanists. The five largest orders, as well for genera as for species, are the following, and in this rank: Compositæ, Leguminosæ, Orchideæ, Rubiaceæ, Gramineæ. The high standing of the orchid family in the list will be a surprise to many. Linnæus knew only a hundred species; five thousand is now a moderate estimate—about half as many as there are of Compositæ, which hold to their proportion of one-tenth of the whole. In both families every country and district is largely peculiar in its species and types. The far greater prominence of Compositæ over orchids is owing to the vast number of individuals in the former, their paucity in the latter.

Those who desire to know the respective parts which the two authors have taken in the elaboration of the "Genera Plantarum" may be referred to a short article on the subject in a recent number of the Journal of the Linnean Society of London. Great thanks from all botanists are due to them both.—(*Reprint from the Nation*, July 19, 1883.) A. G.

2. *Itinera Principum S. Coburgi* are published in princely style. We have before us the first volume of the Botany of the Travels of (1.) The Princes Philip and Augustus of Saxe-Coburg-Gotha round the world, in 1872–73, and (2.) The Visit to Brazil of the Princes Augustus and Ferdinand in 1879. Edited by Dr. H. Wawra von Fernsee, and published at Vienna in 1883. It is an imperial quarto volume of 182 pages, in beautiful typography, and with 39 charming lithographic plates, most of them hand-colored. The plants enumerated, and in case of new or critical ones described, are of various orders of Dicotyledones (but Compositæ omitted from this volume), and of the *Bromeliaceæ* among the *Monocotyledones*. To this order special attention is paid, and twenty of the plates are devoted to its illustration. The voyage and journey round the world was by way of New York and by the Central Pacific Railway to Utah and California, including the customary visit to the Yosemite and the Big Trees, thence to the Sandwich Islands, New Zealand, Australia, Ceylon, a turn to Japan, thence to Java, to India, Suez, and so to Trieste. We need not rehearse the itinerary of the subsequent Brazilian expedition. A second volume is to follow the first. The interest for us centers mainly in the *Bromeliaceæ* from Brazil, which are treated in detail, and interesting new forms figured; also in the small collection of North American plants, picked up *en route* across the Continent and in California. No new species are noted, but several give occasion for critical remarks or descriptions. A. G.

3. *Notice Biographique sur M. Joseph Decaisne*, par *Edouard Bornet*.—This very interesting and appreciative memorial was printed as a preface to the Catalogue of the Library of Professor

Decaisne, but is reprinted as a pamphlet of 19 pages, 8vo. It contains considerable information which we had not elsewhere met with, and is a worthy tribute to the memory of an admirable man and eminent botanist. A. G.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Supplementary note on the Bishopville Meteorite.* — Since the publication of my paper relating to the Bishopville and Waterville meteorites (this Journal for July), Professor C. U. Shepard, Sen., has called my attention to a foot note given in a paper of his entitled "Contributions to Mineralogy," and published at Amherst, Mass., in 1877.

In this note Professor Shepard claimed that his chladnite had "very erroneously been confounded with bronzite;" and that "it now seems probable that the blue substance [iodolite] was some spontaneously decomposable compound of S with other elements of the stone. The same may have been the case with the yellow mineral [apatoid] therein detected." M. E. W.

Cambridge, Mass., July 23, 1883.

2. *Lectures delivered [free] to the employés of the Baltimore and Ohio Railroad Company;* by Professor H. NEWELL MARTIN and Drs. HENRY SEWALL, WM. T. SEDGWICK and WM. K. BROOKS, of the Johns Hopkins University. 98 pp. 8vo. Baltimore, 1882.—These four lectures are thorough in their scientific character and, at the same time, well adapted by clearness in style and illustration to the audience to which they were delivered. A more scientific audience might have profited by them. The subjects are: How skulls and backbones are built; How we move; On Fermentation; Some curious kinds of Animal locomotion. The pamphlet was printed at the expense of President John W. Garrett, of the Railroad Company, for free distribution among the employés of the road, and "copies can be secured by written or personal application at the President's office in Baltimore."

3. *The Iroquois Book of Rites.* Edited by HORATIO HALE, M.A., author of the Ethnography and Philology of the Wilkes U. S. Exploring Expedition, etc. 222 pp. 8vo. Philadelphia, 1883 (D. G. Brinton).—This important contribution to American history before the time of Columbus, and to the science of human progress, is by a man of wide learning and careful research, after special investigations among the Indians of the Iroquois tribe at the reservations in Canada and New York. It constitutes vol. ii of the "Library of Aboriginal American Literature."

4. The meeting of the *American Association* opened on Wednesday, the 15th of August. As the sessions are still in progress while these pages are printing, a notice of its proceedings is necessarily deferred.

Memoir of Louis Agassiz, by ARNOLD GUYOT. 50 pp. 8vo, 1883. Read before the National Academy of Sciences.—An admirable memoir by one who was a companion of Professor Agassiz from his childhood. in the Neufchatel University, and in the decision to leave the University and Switzerland for the freer land of America.

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

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ART. XXIX.—*On some controverted points in Geological Climatology; a reply to Professor Newcomb, Mr. Hill and others;* by JAMES CROLL, LL.D., F.R.S.

NINETEEN years ago the theory was advanced that the Glacial epoch was the result of a combination of physical agents brought into operation by an increase in the eccentricity of the earth's orbit. Few or no objections have been urged against what may be called the astronomical part of the theory. But the portions relating to these physical agencies, which is by far the most important part, have from time to time met with considerable opposition. Considering the newness of the subject, and the complex nature of many of these combinations of physical agencies, it would not be surprising if some of the original deductions in regard to them proved erroneous. But after long and careful re-consideration of the whole matter I have not found reason to abandon any of them or alter them to any material extent.

The only class of objections urged against the theory which I have as yet considered at length are those relating to the cause of ocean currents, and their influence on the distribution of heat over the globe; and I think, it will be admitted that the views which I have advocated on these points are now being pretty generally accepted.

But it is in reference to the influence of aqueous vapor, fogs and clouds on the production and preservation of snow that the greatest diversity of opinion has prevailed. The object of the present article is to examine at some length the principal objections which have been advanced in regard to this part of the inquiry. I shall also take the present opportunity of discussing more fully some points on which I have been sometimes misunderstood, and which appear to have been treated rather too briefly on former occasions.

In the American Journal of Science for April, 1876, Professor Newcomb has done me the honor to review at some length my work, 'Climate and Time.' And as his article is mainly devoted to a criticism of my reasoning in regard to those very points to which I refer, I shall begin with an examination of his objections. One reason for entering at some length into an examination of Professor Newcomb's objections is the fact that they embrace to a large extent those which have been urged by reviewers in Great Britain. Some of his objections, however, as will be seen, are based upon a misapprehension of my reasoning.

Temperature of Space.—One of the most important factors in the theory of geological climate resulting from changes in the eccentricity of the earth's orbit is obviously the temperature of stellar space. Unless we have, at least, some rough idea of the proportion which the heat derived from the stars bears to that derived from the sun, we can not form any estimate of how much the temperature of our earth would be lowered or raised by a given decrease or increase of the sun's distance.

The question of the temperature of space has been investigated in different ways by Pouillet and Herschel; and the result arrived at was that space has a temperature of −239° F., or an absolute temperature of 222°. The mean absolute temperature of our earth is about 521°. Consequently, according to these results, the heat received from the stars is to that received from the sun as 222 to 299. All my determinations of the change of temperature due to changes in the sun's distance were computed on these data, although I believe, for reasons stated, that space must have a much lower temperature. Recent observations of Professor Langley made during the Mount Whitney Expedition confirm the correctness of my belief.

Professor Newcomb, however, wholly ignores all that has been done on that subject, for he commences his review by the statement that "practically there is but one source from which the surface of the earth receives heat—the sun, since the quantity received from all other sources is quite insignificant in comparison."

Surely Professor Newcomb must have forgotten all about the researches of Pouillet and Herschel into what has been termed the 'Temperature of Space,' or he could not have affirmed so positively that "practically there is but one source from which the earth receives heat, and that all other sources are quite insignificant" without, at least, giving some reason for the assertion.

I am pleased to find that he agrees, in the main, with what has been advanced in 'Climate and Time,' in reference to the heating power of ocean currents, and also as to their existence being due to the impulse of the winds. But he differs widely from me in regard to the heat conveyed by aerial currents.

On the Heat conveyed by Aerial Currents.—I stated that the quantity of heat conveyed from equatorial to high temperate and polar regions is trifling in comparison with that conveyed by ocean currents. For the heated air rising off the hot ground of the equator, after ascending a few miles becomes exposed to the intense cold of the upper regions, and having to travel polewards for thousands of miles in those regions, it loses nearly all the heat which it brought from the equator before it can possibly reach high latitudes. To this Professor Newcomb objects as follows: "He (Mr. Croll), speaks of the hot air rising from the earth and becoming exposed to the intense cold of the upper regions of the atmosphere. But what can this cold be *but the coldness of the very air itself which has been rising up?* If the warm air rises up into the cold air, and becomes *cooled by contact* with the latter, the latter must become warm by the very heat which the former loses, and if there is a continuous rising current the whole region must take the natural temperature of the rising air. This temperature is, indeed, much below that which maintains at the surface *for the simple reason that air becomes cold by expansion* according to a definite and well-known law. Having thus got his rising current constantly cooled off *by contact with the cold air* of the upper regions, it has to pass on its journey towards the poles," etc., p. 267.*

Here the cooling of the ascending air is attributed to two causes (1) the heat lost by expansion as the air rises; (2) the heat lost by contact with the colder air through which the ascending air passes and with which it mixes in the upper regions. But the two may be resolved into one, viz: the heat lost by expansion; for the cold air to which the ascending air communicates its heat by contact, is assumed to have originally derived its cold, in like manner from expansion. This is evident, for although he recognizes the effect of radiation into space, he assumes that this loss is compensated by counter radiation. The upper regions are, he says, exposed to the radiation of the sun on the one side, and of the earth's lower atmosphere on the other, and there is no proof that these do not equal the surface temperature. And again, when the air descends in high latitudes to the earth's surface an amount of heat will be evolved by compression equal to that which it lost when it rose from the equator.

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* The italics are mine.

Professor Newcomb has misapprehended not only my meaning, but also the chief reason why the air in the upper regions is so intensely cold. Any one who has read what I have stated in pp. 35-40, 'Climate and Time,' regarding the temperature of space will readily understand what I mean by the temperature of the upper regions. By the temperature of stellar space, it is not meant that space itself is a something possessed of a given temperature, say −239° F. It simply means the temperature to which a body would fall were it exposed to no other source of heat than that of radiation from the stars. By the temperature of the upper regions, I mean the temperature to which air in those regions sinks in consequence of loss from radiation into space. It is mainly to this cause, and not to the loss from expansion, as Professor Newcomb assumes, that the intense cold of the upper air is due. The air in that region has got beyond the screen which protected it when at the earth's surface, and it then throws off its heat into space during twelve hours of night, getting no return from without except from the radiation of the stars. And even at noon-day, as I have endeavored to show in Appendix, p. 551, the rays of a burning sun over head would not be sufficient to raise the temperature of the air up to the freezing point. But the recent observations of Professor Langley prove that the loss of heat from radiation is in reality far greater than I had anticipated. He says: "The original observations, which will be given at length, lead to the conclusion that in the absence of an atmosphere the earth's temperature of insolation would at any rate fall below −50° F., by which it is meant that, for instance, mercury would remain a solid under the vertical rays of a tropical sun were radiation into space wholly unchecked, or even if, the atmosphere existing, it let radiations of all wave-lengths pass out as easily as they come in."—'*Nature*,' Aug. 3d, 1882.

The temperature of the upper atmosphere, even after making allowance for heat received from below, must in this case be at least nearly 80 degrees below the freezing point. The quantity of heat lost by expansion must, therefore, be trifling compared with that lost by radiation; and although the heat lost by expansion is fully restored by compression, yet the air would reach the earth deprived almost entirely of the heat with which it left the equator. All that it could possibly give back would simply be the heat of compression, and this would hardly be sufficient to raise air at −50° F. to the freezing point. How then can the polar regions be greatly the better of air from the equatorial regions? Professor Newcomb says: “If the upper current be as great as is commonly supposed, it must be as powerful as ocean currents in tending to equalize the temperature of the globe.” How can this be?

Why the mean temperature of the Ocean should be greater than that of the Land.—“Another proposition,” he says, “which the author attempts to prove, reasoning which seems equally inconclusive, is that the mean temperature of the ocean is greater than that of the land over the entire globe.” I certainly never attempted to prove that the mean temperature of the ocean is *greater* than that of the land over the entire globe. The very chapter to which he here refers, and which he is about to criticise, was written to explain why the mean temperature of the southern or water hemisphere is less than that of the northern or land hemisphere. What I attempted o prove was, not that the mean temperature of the ocean s greater than that of the land, but that were it not for ertain causes the mean temperature of the ocean *ought* to be greater than that of the land in equatorial regions as well as n temperate and arctic regions. In other words, the object of he chapter is to prove that the mean temperature of the outhern or water hemisphere is less than that of the northern r land hemisphere, not, as is generally supposed, because the ormer is mainly water and the latter land, but because of the normous amount of heat transferred from the former to the atter hemisphere by means of ocean currents; and that were t not for this transference the temperature of the water would xceed that of the land hemisphere. And it is in order to prove this that the “four *à priori* reasons” which Professor Newcomb criticises were adduced. The first of these is as ollows—

> *First.*—‘The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles and its transparency for heat-rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great.’*

These sentences are considered unworthy of criticism. Are they really so unworthy? Let us examine them a little more closely. It is in consequence of the sun's rays being able to penetrate to a great depth that the amount of heat stored up by the ocean is so great, and it is to this store that its warmth during winter is mainly due. The water is diathermanous for the rays of the sun, but it is not so, for reasons well known, for the rays of water itself. The upper layers of the ocean will allow a larger portion of the radiation from the sun to pass freely downward, but they will not allow radiation from the layers underneath to pass freely upwards. These upper layers, like the glass of a green-house, act as a trap to the sun's rays, and thus allow the water of the ocean to stand at a higher temperature than it would otherwise do. Again, the slowness with which the ocean thus parts with its heat enables it to maintain that comparatively high temperature during the long winter months. And again, it is to the mobility of the particles of water, the depth to which the heat penetrates, and the rapidity with which it is absorbed, that those great currents of warm water become possible. Were the waters of the ocean like the land not mobile, and were only a few inches at the surface reached by heat from the sun, there could be no Gulf Stream, or any great transference of heat from the Southern to the Northern hemisphere, or from equatorial to temperate and polar regions, by means of oceanic circulation.

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* ‘Climate and Time,’ p. 90.

Second.—'The air is probably heated more rapidly by contact with the ground than with the ocean; but, on the other hand, it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapor of the air is to a great extent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated.'

To this Professor Newcomb objects as follows: "If then the air is really heated by contact with the ground more rapidly than by contact with the ocean, it can only be because the ground is hotter than the ocean, which is directly contrary to the theory Mr. Croll is maintaining." What I maintained was that were it not for certain causes the *mean annual* temperature of the ocean would be higher than that of the land. During the day and also during the summer the surface of the ground is hotter than that of the ocean; and the air, of course, will be heated more rapidly by *contact* with the former than with the latter. But this does not prove that the air is not more rapidly heated by *radiation* from the ocean than from the land. Professor Newcomb says: "The statement that the aqueous vapor of the air is diathermanous to radiation from land, but not to that from water, is quite new to us, and very surprising." I am surprised that he is not acquainted with the fact, and also with its physical explanation. This will elp to account for his inability to perceive how radiation from he ocean may heat the air more rapidly than radiation from he land, even though the surface of the latter may be at a higher temperature than that of the former.

He says: "The rapidity with which the heating process goes on depends on the difference of temperature, no matter whether the heat passes by conduction or by radiation." This statement will hardly harmonize with recent researches into radiant heat. It is found that the rapidity with which a body is heated by radiation depends upon the absorbing power of the body; and the absorbing power again depends upon the quality of the heat rays. Professor Tyndall, for example, found that in the case of vapors, as a rule, absorption *diminishes* as the temperature rises. With a platinum spiral heated till it was barely visible, the absorption of the vapor of bisulphide of carbon was 6·5, but when the spiral was raised to a white heat, the absorption was reduced to 2·9. A similar result took place in the case of chloroform, formic ether, acetic ether, and other vapors. The physical cause of this is well known.

If the aqueous vapor of the air, he says, be more diathermanous to radiation from land than from water, as I have stated, then I assigned directly contrary effects to the same cause. For, "reasoning as in (1) he, Mr. Croll, would have said that the air over the land, owing to its transparency for the heat rays from the land, becomes heated to a greater height rapidly, while the air over the ocean not being transparent, can acquire heat from the ocean only by the slow process of convection." I would have said no such thing. Radiation from the surface of the land will, no doubt, penetrate more freely through the aqueous vapor than radiation from the ocean, but the aqueous vapor will not absorb the radiation of the land so rapidly as that of the ocean, for the ocean gives off that quality of rays which aqueous vapor absorbs most rapidly.

This is not in opposition to what I have stated in reason (1), for if the ground were transparent to the sun's rays like water, evidently the total quantity of heat absorbed by it would be greater than that by the ocean. But radiation from the sun heats only the surface of the ground, all below the surface depends for its supply on the slow process of conduction, whereas the ocean is heated by direct radiation to great depths. Consequently the total quantity of heat absorbed by he ocean, say per square mile, in a given time, is greater than hat by the land.

Third.—'The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does. The ocean will not reflect the heat from the aqueous vapor of the air, but absorbs it, while the ground does the opposite. Radiation from the air, therefore, tends more readily to heat the ocean than it does the land.'

"Here we have," he says, "the air giving back to the ocean the same heat which it absorbs from it, and thus heating it." If Professor Newcomb means by this same heat the same amount of heat, then I believe in no such thing. But if his meaning be that here we have the air giving back to the ocean a quantity of the heat which it absorbed from it, then he is certainly correct in supposing that this is affirmed by me. But this is a conclusion which no physicist could for a moment doubt. To deny this would be to contradict Prevost's well-known theory of exchanges. Did the air throw back to the ocean none of the heat which it derives from it the entire waters of the ocean would soon become solid ice. In fact, as we have seen, mercury would not remain fluid and every living thing on the face of the globe would perish.

He states that reason fourth seems to be little more than a repetition of reason second in a different form. It is, however, much more than that. It is a demonstration that were it not for the causes to which I have alluded the mean temperature of the water hemisphere ought to be higher than that of the land hemisphere, and for this reason I shall here give the section in full.

Fourth.—'The aqueous vapor of the air acts as a screen to prevent the loss by radiation from water, while it allows radiation from the ground to pass more freely into space; the atmosphere over the ocean consequently throws back a greater amount of heat than is thrown back by the atmosphere over the land. The sea in this case has a much greater difficulty than the land has in getting quit of the heat received from the sun; in other words, the land tends to lose its heat more rapidly than the sea. The consequence of all these circumstances is that the ocean must stand at a higher mean temperature than the land. A state of equilibrium is never gained until the rate at which a body is receiving heat is equal to the rate at which it is losing it; but as equal surfaces of sea and land receive from the sun the same amount of heat, it therefore follows that in order that the sea may get quit of its heat as rapidly as the land, it *must stand at a higher temperature* than the land. The temperature of the sea must continue to rise till the amount of heat thrown off into space equals that received from the sun; when this point is reached equilibrium is established and the temperature remains stationary. But, owing to the greater difficulty that the sea has in getting rid of its heat, the mean temperature of equilibrium of the ocean must be higher than that of the land; consequently the mean temperature of the ocean, and also of the air immediately over it, in tropical regions should be higher than the mean temperature of the land and the air over it.'

Since the publication of 'Climate and Time,' the accuracy of this conclusion has been confirmed in a remarkable manner from recent researches on the actual mean temperature of the two hemispheres, the details of which have been given by Mr. Ferrel in his 'Meteorological Researches,' Washington, 1877. It is found that the mean temperature of the northern or land hemisphere is higher than that of the southern or water hemisphere up only to about latitude 35°, and that beyond this latitude the mean temperature of the water hemisphere is the greater of the two. At latitude 40° the mean temperature of the southern hemisphere is 1°·4 higher than that of the same parallel on the northern hemisphere. At latitude 50° the difference amounts to 4°·4, while at latitude 60° the mean temperature of the southern hemisphere is actually 6° higher than that of the northern on the same parallel. The mean temperatures of the two hemispheres are as follows:

Lat.,	0°	10°	20°	30°	40°	50°	60°	70°	80°
Northern,	80°·1	81°·0	77°·6	67°·6	56°·5	43°·4	29°·3	14°·4	4°·5
Southern,	80°·1	78°·7	74°·7	66°·7	57°·9	47°·8	35°·3	....	....

From the above table we see that it is only in that area lying between the equator and latitude 35° that the southern hemisphere has a lower mean temperature than the northern. But it is from this area that the enormous amount of heat transferred to the northern hemisphere is mainly derived. Were the transference of heat to cease the temperature of this area would be very considerably raised, and that of the corresponding area on the northern hemisphere lowered. The result would doubtless be that the southern hemisphere, down to the equator, would then be warmer than the northern. But even as things are, as Mr. Ferrel remarks, "the mean temperature of the southern hemisphere is the greater of the two," the mean temperature of the southern being 60°·89, and that of the northern 59°·54.

Heat cut off by the Atmosphere.—Professor Newcomb says further: "Another idea of the author which calls for explanation is that solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned." This is no idea of mine. My idea is not that the heat cut off is entirely lost but merely that the *greater part* is lost. A large portion of the heat is reflected, and of that absorbed one-half, perhaps, is radiated back into space and lost, in so far as the earth is concerned.

Tables of Eccentricity.—Referring to my tables of eccentricity of the earth's orbit he says: "That there are from time to time such periods of great eccentricity is a well-established result of the mutual gravitation of the planets, but whether the particular epochs of great and small eccentricity computed by Mr. Croll are reliable, is a different question." I may here mention that Professor McFarland, of the Ohio State University, Columbus, a few years ago, undertook the task of re-computing every one of the 150 periods given in my tables, and he states that, except in one instance, he did not find an error to the amount of ·001.[*]

"The data for this computation," continues Professor Newcomb, "are the formulæ of Le Verrier, worked out about 1845,[†] without any correction either for the later corrections to the masses of the planets or for the terms of the third order, subsequently discussed by Le Verrier himself. The probable magnitude of these corrections is such that reliance cannot be placed upon the values of eccentricity computed without reference to them for epochs distant by merely a million of years."

In regard to this objection I may mention that the whole subject of the secular variations of the elements of the planetary orbits has been re-investigated by Mr. Stockwell, taking into account the disturbing influence of the planet Neptune, the existence of which was not known at the time Le Verrier's investigations were made. Professor McFarland, with the aid of Mr. Stockwell's formulæ, has computed all the periods in the tables referred to above, and on comparing the results found by both formulæ, he states that "the two curves exhibit a general conformity throughout their whole extent." And his computations, I may state, extend from 3,260,000 years before 1850 to 1,260,000 years after that date; or, in other words, over a period of no fewer than 4,520,000 years,[‡] thus showing that Professor Newcomb's objection falls to the ground.

Influence of Winter in Aphelion.—I have maintained that at a time when the eccentricity is high and the winter occurs in aphelion, the great increase in the sun's distance and in the length of the winter would have the effect of causing a large increase in the quantity of snow falling during that season. This very obvious result follows as a necessary consequence from the fact that the moisture which now falls in the form of rain would then fall as snow. But Professor Newcomb actually states that he cannot accept the conclusion that this would lead to more snow.

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[*] American Journal of Science. vol. xi. p. 456 (1876).

[†] Le Verrier's formulæ were worked out several years before 1845.

[‡] In this laborious undertaking Professor McFarland computed, by means of both formulæ, the eccentricity of the earth's orbit and the longitude of the perihelion for no fewer than 485 separate epochs. See American Journal of Science, vol. xx. p. 105 (1880).

Influence of a Snow-covered Surface.—I have argued that this accumulation of snow would lower the summer temperature, and tend to prevent the disappearance of the snow, and have assigned three reasons for this conclusion :—

First.—Direct radiation. The snow, for physical reasons well known, will cool the air more rapidly than the sun's rays will heat it. This is shown from the fact that in Greenland, a snow and ice-covered country, a thermometer exposed to the direct radiation of the sun has been observed to stand above 100°, while the air surrounding the instrument was actually 12° below the freezing point. Professor Newcomb and also Mr. Hill [*] regard the idea that this could in any way favor the accumulation of snow as absurd. They think that in fact it would have directly the opposite effect. They have perceived only one half of the result. It is quite true, as they affirm, that the cooling of the air by the snow will not prevent the melting of the snow, but the reverse. There is, however, another and far more important result overlooked in their objection. If the snow and ice-covered surface keeps the temperature of the air, in summer, below the freezing point, which it evidently does in Greenland and in the Antarctic continent, the moisture of the air will fall as snow and not as rain. No doubt this is the chief reason why in those regions, even in the middle of summer, rain seldom falls, the precipitation being almost always in the form of snow, although at that very season the direct heat of the sun is often as great as in India. Were the snow and icy mantle removed, a snow shower in summer would be as rare a phenomenon in those regions as it would be in the south of England.

Second.—'The rays which fall on snow and ice are to a great extent reflected back into space. But those that are not reflected, but absorbed, do not raise the temperature, for they disappear in the mechanical work of melting the ice.'

This reason is also regarded as absurd. The heat of the sun during the perihelion summer would, he says, suffice to melt the whole accumulation of winter snow in three or four days. "The reader," he continues, "can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months." Incredible as it may appear to Professor Newcomb, I shall shortly show that a less amount of snow than the equivalent of the two feet of ice which he assumes does actually, in some places, defy the melting power of a tropical sun. But he misapprehends my reasoning here also, by overlooking the more important factor in the affair, namely, the keeping of the air in the summer below the freezing point. The direct effect that this has in preventing the sun from melting the snow and ice will be discussed shortly, but the point to which I wish at present to direct special attention is the fact that if the air is kept below, or even at the freezing point, snow will fall and not rain. Snow is a good reflector of heat, consequently a large portion of the sun's rays falling on the snow and icy surface is reflected back to space. The aqueous vapor of the air, on the other hand, as the vibrations of its molecules agree in *period* with those of the snow and ice, cuts off a large portion of the heat *radiated* by the snow surface; but here in the case of reflection under consideration the rays are not cut off; for the *reflected* rays are of the same character as the incident rays which pass so freely through the aqueous vapor. And in respect to the remaining rays which are not reflected but absorbed by the snow, they do not manage to raise the temperature of the snow above the freezing point. Consequently the air is kept in the condition most favorable for the production of snow.

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[*] Geological Magazine for January, 1880, p. 12.

Third.—'Snow and ice lower the temperature by chilling the air and condensing the vapor into thick fogs. The great strength of the sun's rays during summer, due to his nearness at that season, would, in the first place, tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy sea would chill the atmosphere and condense the vapor into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the snow in consequence, would remain unmelted during the entire summer.'

On this Professor Newcomb's criticism is as follows: "Here he, Mr. Croll, says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled. The task of arguing with a disputant who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the next breath that thick fogs effectually prevent the rays ever reaching the snow at all, is not free from embarrassment."

If he really supposes my meaning to be that the air is so transparent as to allow the incident and reflected rays of the sun to pass freely without interruption, while at the same time and in the same place the air is not transparent but filled with dense fogs which effectually cut off the sun's rays and prevent them from reaching the earth, then I do not wonder that he should feel embarrassed in arguing with me. But if he supposes my meaning to be, as it of course is, that those two opposite conditions, existing at totally different times, or in totally different places at the same time, should lead to similar results, namely, the cooling of the air and consequent conservation of snow, then there is no ground whatever for any embarrassment about the matter.

"We might therefore show," he states, "that if the snow, air, fog, or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator, and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter."

There are few points in connection with terrestrial physics which appear to be so much misunderstood as that of the influence of fogs on climate. One chief cause of these misapprehensions is the somewhat complex nature of the subject arising from the fact that aqueous vapor acts so very differently under different conditions. When the vapor exists in the air as an invisible gas, we have often an intensely clear and transparent sky, allowing the sun's rays to pass to the ground with little or no interruption; and if the surface of the ground be covered with snow, a large portion of the incident rays are reflected back into space without heating either the snow or the air. The general effect of this loss of heat is, of course, to lower the general temperature. But when this vapor condenses into thick fogs it acts in a totally different manner. The transparency to a great extent disappears, and the fog then cuts off the sun's rays and prevents them from reaching the ground. This it does in two different ways. 1st. Its watery particles, like the crystals of the snow, are good reflectors, and the upper surface of the mass of fog on which the rays fall acts as a reflector, throwing back a large portion of the rays into stellar space. The rest of the rays which are not reflected enter the fog and the larger portion of them are absorbed by it. But it will be observed that by far the greater part of the absorption, if not nearly all of it, will take place in the upper half of the mass. This is a necessary result of a recognized principle in radiant heat known as the "sifting" of the rays. The deeper the rays penetrate into the fog the less will be the amount of heat absorbed. If the depth of the mass be great, absorption will probably entirely disappear before the surface of the ground is reached. The fog will begin, of course, to radiate off the heat thus absorbed, but as it is the upper half of the mass which has received the principal part of the heat, the most of this heat will be radiated upward into stellar space, and like the reflected heat entirely lost in so far as heating the earth is concerned. A portion will also be radiated downward, some of which may reach the ground, but the greater portion will be re-absorbed in its passage through the mass. We have no means of estimating the amount of heat which would thus be thrown off into space by reflection and radiation ; but it is certainly great. I think we may safely conclude that in places like South Georgia and Sandwich Land where fogs prevail to such an extent during summer, one half at least of the heat from the sun never reaches the ground. A deprivation of sun heat of a much less extent than this would certainly lower the summer temperature of these places far below the freezing point, were it not for a compensating cause to which I shall now refer, viz : the heat "trapped" by the fog. The fog, although it prevents a large portion of the sun's heat from ever reaching a place, at the same time prevents to a great extent that place from losing the little heat which it does receive. In other words, it acts as a screen preventing the loss of heat by radiation into space. But the heat thus "trapped" never fully compensates for that not received, and a lowering of temperature is always the result.

Had all these considerations been taken into account by Professor Newcomb, Mr. Hill, Mr. Searles Wood and others, they would have seen that I had by no means over-estimated the powerful influence of fogs in lowering the summer temperature.

The influence of fogs on the summer temperature is a fact so well established by observation that it seems strange that any one should be found arguing against it.

Heat evolved by Freezing.—There is one objection to which I may here refer and which has been urged by nearly all my critics. It is said, correctly enough, that as water in freezing evolves just as much heat as is required to melt it, there is on the whole no actual loss of heat ; that whatever heat may be absorbed in the mechanical work of melting the snow just as much was evolved in the formation of the snow. Consequently it is inferred, in so far as climate is concerned, the one effect completely counterbalances the other. This inference, sound as it may at first sight appear, has been so well proved to be incorrect by Mr. Wallace that I cannot do better than quote his words :

"In the act of freezing, no doubt water gives up some of its heat to the surrounding air, but *that air still remains below the freezing point* or freezing would not take place. The heat liberated by freezing is, therefore, what may be termed low-grade heat—heat incapable of melting snow or ice; while the heat absorbed while ice or snow is melting is high-grade heat, such as is capable of melting snow and supporting vegetable growth. Moreover, the low-grade heat liberated in the formation of snow is usually liberated high up in the atmosphere, where it may be carried off by winds to more southern latitudes, while the heat absorbed in melting the surface of snow and ice is absorbed close to the earth and is thus prevented from warming the lower atmosphere, which is in contact with vegetation. The two phenomena, therefore, by no means counterbalance or counteract each other, as it is so constantly and superficially asserted that they do."—*Island Life*, p. 140.

The Fundamental misconception.—I come now to a misapprehension which more than any other has tended to prevent a proper understanding of the causes which lead to the conservation by snow. Whatever the eccentricity of the earth's orbit may be the heat received from the sun during summer is more than sufficient to melt the snow of winter. Consequently, it is assumed, no permanent accumulation of snow can take place. This objection, as expressed by Mr. Hill, is as follows: "We have no reason to suppose that at present in the northern hemisphere more snow or ice is anywhere formed in winter than is melted in summer. With greater eccentricity less heat than now would be received in winter, but exactly as much more in summer. More snow would therefore be formed in the one half of the year, but exactly as much more be melted in the other half. The colder winter and the warmer summer would exactly neutralize each other's effects, and on the average of years no accumulation could begin. *Prima facie*, therefore, high eccentricity will not account for glacial periods."[*] In the language of Professor Newcomb it is as follows: "During this perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt from four to six inches of ice per day over its entire surface; that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months."

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[*] Geological Magazine, January, 1880, p. 12.

It is assumed, in this objection, that because the heat received from the sun by an area is more than sufficient to melt all the snow that falls on it, no permanent accumulation of snow and ice can take place. It is assumed that the quantity of snow and ice melted must be proportional to the heat received. Suppose that on a certain area a given amount of snow falls annually. The amount of heat received from the sun per annum is computed, and after the usual deduction for that cut off by the atmosphere has been made, if it be found that the quantity remaining is far more than sufficient to melt the snow, it is then assumed that the snow must be melted, and that no accumulation of snow and ice year by year in this area is possible. To one approaching this perplexing subject for the first time such an assumption looks very plausible, but a little reflection will show that it is most superficial. The assumption is at the very outset totally opposed to known facts. Take the lofty peaks of the Himalayas and Andes as an example. Few, I suppose, would admit that at these great elevations as much as fifty per cent of the sun's heat could be cut off. But if fifty per cent reaches the snow this would be sufficient to melt fifty feet of ice, and this no doubt is more than ten times the quantity which actually requires to be melted. Notwithstanding all this the snow is never melted but remains permanent. Take as another example South Georgia in the latitude of England. Suppose we assume that one-half of the sun's heat is cut off by the clouds and fogs which prevail to such an extent in that place, still the remaining half would be sufficient to melt upwards of thirty feet of ice, which is certainly more than the equivalent of all the snow which falls; yet this island is covered with snow and ice down almost to the seashore during the whole year. Take still another example, that of Greenland. The quantity of heat received between latitudes 60° and 80°, which is that of Greenland, is, according to Meech, one-half that received at the equator; and were none cut off, it would be sufficient to melt fifty feet of ice. The annual precipitation on Greenland in the form of snow and rain, according to Dr. Rink, amounts to only twelve inches, and two inches of this he considers is never melted but is carried away in the form of icebergs. Mr. Hill maintains* that owing to the great thickness of the air traversed by the sun's rays and the loss resulting from the great obliquity of reflection, the amount of heat reaching the ground would be insufficient to melt more than sixteen feet o ice. Supposing we admit this estimate to be correct still thi is nineteen times more than is actually melted. The sun melt only ten inches notwithstanding the fact that it has the power to melt sixteen feet.

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* Geological Magazine, April, 1880.

In short, there is not a place on the face of the globe where the amount of heat received from the sun is not far more than sufficient to melt all the snow which falls upon it. If it were true, as the objection assumes, that the amount of snow melted is proportional to the amount of heat received by the snow then there could be no such thing as perpetual snow.

The reason why the amount of snow and ice melted is not necessarily proportional to the amount of heat received is not far to seek. Before snow or ice will melt its temperature must be raised to the melting point. No amount of heat, however great, will induce melting to begin unless the intensity of the heat be sufficient to raise the temperature to the melting point. Keep the temperature of the snow below that point and though the sun may shine upon it for countless ages it will still remain unmelted. It is easy to understand how the snow on the lofty summits of the Himalayas and the Andes never melts. According to the observations made at Mount Whitney, to which reference has already been made, the heat of even a vertical sun would not be sufficient at these altitudes to raise the temperature of the snow to near the melting point; and thus melting, under these conditions, is impossible. The snow will evaporate but it cannot melt. But owing to the frozen condition of the snow even evaporation will take place with extreme difficulty. If the sun could manage to soften the snow crystals and bring them into a semi-fluid condition, evaporation would, no doubt, go on rapidly; but this the rays of the sun are unable to do. Consequently, we have only the evaporation of a solid which, of course, is necessarily small.

It may here be observed that at low elevations, where the snow fall is probably greater, and the amount of heat received even less than at the summits, the snow melts and disappears. Here, again, the influence of that potent agent, aqueous vapor, comes into play. At high elevations the air is dry and allows the heat radiated from the snow to pass into space; but at low elevations a very considerable amount of the heat radiated from the snow is absorbed by the aqueous vapor which it encounters in passing through the atmosphere. A considerable portion of the heat thus absorbed by the vapor is radiated back on the snow; but the heat thus radiated being of the same quality as that which the snow itself radiates is on this account absorbed by the snow. Little or none of it is reflected like that received from the sun. The consequence is that the heat thus absorbed accumulates in the snow till melting takes place. Were the amount of aqueous vapor possessed by the atmosphere sufficiently diminished, perpetual snow would cover our globe down to the sea shore. It is true that the air is warmer at the lower than at the higher levels, and by contact with the snow must tend to melt it more at the former than at the latter position. But we must remember that the air is warmer mainly in consequence of the influence of aqueous vapor, and that were the quantity of vapor reduced to the amount in question, the difference of temperature at the two positions would not be great.

But it may be urged as a further objection to the foregoing conclusion that, as a matter of fact, on great mountain chains the snow line reaches to a lower level on the side where the air is moist than on the opposite side where it is dry and arid: as, for example, on the southern side of the Himalayas, and on the eastern side of the Andes, where the snow line descends 2,000 or 3,000 feet below that of the opposite or dry side.

But this is owing to the fact that it is on the moist side that by far the greatest amount of snow is precipitated. The moist winds of the southwest monsoon deposit their snow almost wholly on the southern side of the Himalayas, and the southeast trades on the east side of the Andes. Were the conditions in every respect the same on both sides of these mountain ranges, with the exception only that the air on one side was perfectly dry, allowing radiation from the snow to pass without interruption into stellar space, while on the other side the air was moist and full of aqueous vapor absorbing the heat radiated from the snow, the snow line would in this case undoubtedly descend to a lower level on the dry than on the moist side. Melting would certainly take place at a greater elevation on the moist than on the dry side, and this is what would mainly determine the position of the snow line.

The annual precipitation on Greenland, as we have seen, is very small; scarcely one-half that of the dryest parts of Great Britain. This region is covered with snow and ice, not because the quantity of snow falling on it is great, but because the quantity melted is small; and the reason why the snow does not melt is not that the amount of heat received during the year is unequal to the work of melting the ice, but that, mainly through the dryness of the air, the snow is prevented from rising to the melting point. The very cause which prevents a heavy snowfall protects the little which does fall from disappearing. The same remarks apply to the antarctic regions.

In South Georgia and Fuegia, where clouds and dense fogs prevail during nearly the whole year, the permanent snow and ice are due to a different cause. Here the snowfall is great and the amount of heat cut off enormous; but this alone would not account for the non-disappearance of the snow and ice. For, notwithstanding this, the heat received is certainly more than sufficient to melt all the snow which falls, great as that amount may be. The real cause is that the heat received is not sufficiently intense to raise the temperature to the melting point. More heat is actually received by the snow than is required to melt it, but it is dissipated and lost before it can manage to raise the temperature of the snow to the melting point; consequently the snow is not melted. Here snow falls in the very middle of summer, but snow would not fall unless the temperature were near the freezing point.

Foregoing principles applied to the case of the Glacial epoch.—Let us now apply the foregoing principles to the case of the Glacial epoch. As winter then occurred in aphelion during a high state of eccentricity, that season would be much longer and colder than at present. Snow in temperate regions would then fall in place of rain, and although the snowfall during the winter might not be great, yet as the temperature would be far below the freezing point, what fell would not melt. As heat, which produces *evaporation*, is just as essential to the accumulation of snow and ice as is cold, which produces *condensation*, after the sun had passed the vernal equinox and summer was approaching the consequent rise of temperature would be accompanied by an increase in the snowfall. A melting of the snow would also begin, but it would be a very considerable time before the amount melted would equal the daily amount of snow falling. Rain, alternating with snow showers, would probably result; and, for some time before mid-summer, snow would cease and give place entirely to rain. Melting would then go on rapidly, and by the end of the summer the snow would all disappear except on high mountain summits such as those of Scotland, Wales and Scandinavia. Before the end of autumn, however, it would again begin to fall. Next year would bring a repetition of the same process, with this difference, however, that the snow line would descend to a lower level than on the previous year. Year by year the snow line would continue to descend till all the high grounds became covered with permanent snow.

It would not require a very great amount of change from the present condition of things to bring about such a result. A simple lowering of the temperature, which would secure that snow instead of rain, should fall for six or eight months in the year would suffice; and this would follow as a necessary result from an increase of eccentricity. Now if all our mountain summits were covered with permanent snow down to a considerable distance, the valleys would soon become filled with local glaciers. In such a case we should then have more than one-half of Scotland, a large part of the north of England and Wales, with nearly the whole of Norway covered with snow and ice. Here a new and powerful agent would come into operation which would greatly hasten on a glacial condition of things. This large snow and ice-covered surface would tend to condense the vapor into snow. It would during summer chill the air and produce dense and continued fogs, cutting off the sun's rays and leading to a state of things approaching to that of South Georgia, which would much retard the melting of the snow.

It is a great mistake, as I have repeatedly shown, to suppose that the perihelion summers of the Glacial epoch could be hot. No snow and ice-covered continent can enjoy a hot summer. This is clearly shown by the present condition of Greenland. Were it not for the ice the summers of North Greenland, owing to the continuance of the sun above the horizon, would be as warm as those of England; but, instead of this, the Greenland summers are colder than our winters, and snow during that season falls more or less nine days out of ten. But were the ice covering removed a snow shower during summer would be as great a rarity as it would be with us. On the other hand, cover India with an ice sheet, and the summers of that place would be colder than those of England.

When the high grounds of Scotland and Scandinavia, with those of the northern parts of America, became covered with snow and ice, and the eccentricity went on increasing, a diminution of the Gulf Stream and a host of other physical agencies, all tending toward a glacial condition of things, would be brought into operation. This would ultimately and inevitably lead to a general state of glaciation without the aid of any of those additional geographical changes of land and water which some have supposed. This will be shown more fully when we come to examine Mr. Alfred R. Wallace's theory.

The Mutual Reaction of the Physical Agents.—Those who think that the agencies to which I refer would not by themselves bring about a glacial condition appear to overlook a most important and remarkable circumstance regarding their mode of operation, to which I have frequently alluded in 'Climate and Time' (pp. 74–77) and other places. The circumstance is this. The physical agencies in question not only all lead to one result, viz: an accumulation of snow and ice, but their efficiency in bringing about this result is actually trengthened by their mutual reaction on one another. In physics the effect reacts on the cause. In electricity and magnetism, for example, cause and effect in almost every case mutually act and react upon each other, but the reaction of the effect tends to weaken the cause. Those physical agents to which I have referred, no doubt, in their mutual actions and reactions obey the same law; but, in reference to *one particular result*, viz: the accumulation and conservation of snow, those mutual reactions strengthen one another. This is not reasoning in a circle, as Mr. Searles Wood supposes; for the reaction of an effect may on the whole weaken the cause, and yet in regard to a particular result it may strengthen it. In the case under consideration the agents not only act in one direction, but their efficiency in acting in that one direction is strengthened by their mutual reactions. This curious circumstance throws a flood of light on the causes which tended to bring about the Glacial epoch.

To begin with we have a high state of eccentricity. This leads to long and cold winters. The cold leads to snow, and although heat is given out in the formation of the snow, yet the final result is that the snow intensifies the cold: it cools the air and leads to still more snow. The cold and snow bring a third agent into play—*fogs*—which act still in the same direction. The fogs intercept the sun's rays, this interception of the rays diminishes the melting power of the sun, and so increases the accumulation. As the snow and ice continue to accumulate, more and more of the rays are cut off; and on the other hand, as the rays continue to be cut off, the *rate* of accumulation increases, because the quantity of snow and ice melted becomes thus annually less and less. In addition, the loss of the rays cut off by the fogs lowers the temperature of the air and leads to more snow being formed, while again the snow thus formed chills the air still more and increases the fogs. Again, during the winters of a Glacial epoch, the earth would be radiating its heat into space. Had this loss of heat simply lowered the temperature, the lowering of the temperature would have tended to diminish the rate of loss; but the result is the formation of snow rather than the lowering of the temperature.

Further, as snow and ice accumulate on the one hemisphere they diminish on the other. This increases the strength of the trade winds on the cold hemisphere and weakens those on the warm. The effect of this is to impel the warm water of the tropics more to the warm hemisphere than to the cold. Suppose the northern hemisphere to be the cold one, then as the snow and ice begin gradually to accumulate, the ocean currents of that hemisphere, more particularly the Gulf Stream, begin to decrease in volume, while those on the southern or warm hemisphere begin *pari passu* to increase.* This withdrawal of heat from the northern hemisphere favors the accumulation of snow and ice, and as the snow and ice accumulate the ocean currents decrease. On the other hand, as the ocean currents diminish, the snow and ice still more accumulate. Thus the two effects in so far as the accumulation of snow and ice is concerned mutually strengthen each other.

The same process of mutual action and reaction takes place among the agencies in operation on the warm hemisphere, only the result produced is diametrically opposite to that produced in the cold hemisphere. On this warm hemisphere action and reaction tend to raise the mean temperature and diminish the quantity of snow and ice existing in temperate and polar regions.

The primary cause of all these physical agencies being set in operation is a high state of eccentricity of the earth's orbit, and with a continuance of that state a glacial epoch becomes inevitable.

The Explanation begins with Winter.—Mr. Hill asks why I always begin in my explanation with the aphelion winter rather than with the perihelion summer. The reason is that the character of the summer is determined by that of the winter and not the winter by that of the summer. It is true that to a certain extent the influence is mutual, but the effect of the summer on the winter is trifling in comparison with that of the winter on the summer. To begin our explanation with the summer would be like beginning at the end of a story and telling it backward.

Herr Woeikof on the cause of Glaciation.—In an article by A. Woeikof on 'Glaciers and Glacial Periods in their relations to Climate' (Nature, March 2d, 1882), it is maintained that the chief cause which leads to the formation of snow, and consequently to a glacial condition, is a low surface-temperature of the sea surrounding or adjoining the land. When the surface temperature of the water much exceeds the freezing-point the vapor, he says, evaporated from the sea and condensed on the land will be rain and not snow, but when the temperature of the water is near the freezing-point, snow will be the result. A diminution, for example, in the heat brought by the Gulf Stream that would very greatly lower the surface temperature of the sea surrounding Great Britain would, he says, bring about a heavy snow-fall and lead to permanent snow and ice. Again he maintains "as there is no reason to suppose that the surface-temperature of the sea would be lower during winter in aphelion and high eccentricity, it follows that there will not be more snow than now in countries where rain is the rule, even in winter, all other things equal."

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* Professor Dana has shown that in North America those areas which at present have the greatest rain-fall are, as a rule, the areas which were most glaciated during the Glacial epoch. Mr. Searles V. Wood maintains that this fact is inconsistent with the theory that the Glacial period was due to the cause to which I attribute it. I am totally unable to comprehend how he arrives at this conclusion. Supposing the Gulf Stream, as I have maintained, were greatly diminished during the Glacial period, still I think it would follow, other things being equal, that the areas which now have the greatest rain-fall would during that period probably have the greatest snow-fall, and consequently the greatest accumulation of ice. The amount of precipitation might be less than at present, but this would not prevent the areas which had the greatest snow-fall from being most covered with ice.

There is surely a fallacy lurking under this theory of M. Woeikof. Snow instead of rain is not, as he supposes, owing to the low temperature of the water from which the vapor is derived, but to the low temperature of the air where the vapor is precipitated. Of course, when the surface of the sea is near the freezing-point, the air over the sea and the adjoining land is usually also not far from the freezing-point, and consequently the precipitation is more likely to be snow than rain. If the air be cold as it generally is over a snow and ice covered country, a high temperature of the adjoining seas, were this possible, would greatly increase the snow-fall because it would greatly augment the quantity of vapor which would be available for snow.

Edinburgh, Scotland, May 1st, 1883.

ART. XXX.—*Communications from the U. S. Geological Survey, Rocky Mountain division.* IV. *On minerals of the Cryolite group recently found in Colorado;* by WHITMAN CROSS and W. F. HILLEBRAND.

IN this Journal, for October, 1882,[*] we announced, in connection with the description of zircon and other minerals from the neighborhood of Pike's Peak, that cryolite and several allied fluorides had been identified from the same region and would form the subject of a paper at an early day. Shortly after the above date there appeared in the "Zeitschrift für Krystallographie," etc., an interesting and valuable paper by Professor P. Groth[†] containing the results of a searching investigation into the crystallographical and chemical properties, not only of cryolite and its alteration products, but also of the few allied minerals occurring independently. The chemical analyses of the above article, which had been previously published apart, were made by J. Brandl* upon material selected and crystallographically examined by Professor Groth. By a critical review of the existing literature, and by renewed investigations in doubtful cases, it was hoped to clear away the uncertainty which had hung about some of the members of the group, and as the available material was, for the most part, far better than had been examined before, it was possible to obtain very satisfactory results.

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[*] Third series, vol. xxiv, p. 281.

[†] P. Groth, "Beiträge zur Kenntniss der natürlichen Fluorverbindungen," Zeitschrift für Krystallographie, vii, pp. 375-388 and 457-493.

In the present paper, frequent reference will necessarily be made to the results of Messrs. Groth and Brandl. Owing to better material, we are in some cases able to give new or supplementary data, and only in one instance, namely, in regard to the composition of pachnolite, is there any discrepancy between our results and those contained in the articles above cited.

Locality and occurrence.—The point at which these minerals occur may be described as at the northeast base of St. Peter's Dome, a small conical peak, due west of Cheyenne Mountain, and near the Pike's Peak toll-road, which is now completed from Colorado Springs to the Seven Lakes, at the base of Pike's Peak. Although several miles in a straight line from the mountain proper, the locality may be considered as within the "Pike's Peak region."

The country rock of this whole district is a coarse, reddish, biotite granite, of a general type which is common in the Colorado Range of the Rocky Mountains, and is, in all probability, a part of the Archæan formation. A specimen collected near St. Peter's Dome shows largely predominant reddish feldspar, brilliant, black biotite, and quartz, the latter rather subordinate, in small grains between the other minerals. The feldspar consists largely of an intergrowth of orthoclase and albite, somewhat after the manner of perthite. The rock disintegrates very readily (l. c., p. 281), and the study of the geognostic relations in the region is much hindered by the gravelly debris which covers many slopes almost entirely.

The cryolite locality lies on the southeast border of the extensive district within which Amazon stone and its associated minerals are so abundantly found in cavities in the granite. In the immediate vicinity astrophyllite and associated zircon occur in granitic veins, and also, as does the arfvedsonite, in veins of white quartz.

The minerals to be described occur in two veins of massive white quartz, and in each case they were discovered by prospectors. The two veins are scarcely more than one-third of a ile apart, and their continuation is so concealed by soil and ebris that it cannot be seen what their relation to each other nd to the other veins may be. They differ, however, so reatly in the minerals they contain and in the manner of oc-rrence of the latter, as to make it improbable that they are nited.

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* J. Brandl, Sitzungsbericht der königl. bayr. Akademie der Wissenschaften zu München, 1882, p. 118, and Annalen der Chemie, ccxiii, p. 1.

In the vein which we will designate vein A, cryolite, pach-olite, thomsenolite, gearksutite, prosopite and probably ralsto-ite appear, with but rare associated minerals. In vein B, on e other hand, prosopite, fluorite and mixed fluorides occur timately associated with zircon, kaolinite and a greenish yel-w mica. As the minerals of the two veins are so distinct, we n best consider them in the groups afforded by the veins emselves.

VEIN A.

At this place a very steep incline was sunk on an outcrop of hite quartz. At about ten feet from the surface a small, lid mass of fluorides, some two feet in thickness, was struck, d below it came massive quartz again. The boundaries of is mass are quite irregular, and its lateral extent is still known.

This small body of mineral, which was no doubt wholly cry-ite originally, is now composed of the same alteration pro-cts which are known in connection with the Greenland volite, some of them in better development than is known in is latter locality. The only associated minerals observed to netrate the cryolite mass are astrophyllite and columbite, the rmer being almost wholly altered to a dull green substance hich is not homogeneous and whose composition is unknown. he astrophyllite springs from the granitic wall of the vein in e form of blades several inches long, and zircon was noticed th it in the mass at the base of the blades, while never seen bedded directly in the cryolite. The columbite is in small, ombic prisms of the type found with the Amazon stone, and curs quite rarely in isolated crystals. The cryolite decom-sition products adjacent to the astrophyllite will be described ter (page 289).

Adjoining the quartz, the cryolite is always decomposed, and generally replaced by a massive mixture of pachnolite and omsenolite, but not infrequently the alteration has gone still ther and a white powder of extreme fineness, like purest olin, fills the space between quartz and more solid pachno-e. This is the gearksutite which, when wet, as it usually is re, makes a thick paste or mud. The description of the nerals will begin with cryolite and proceed through the fferent stages of alteration here exhibited.

CRYOLITE.

Within the mass of fluorides in vein A there is a surprisingly large amount of fresh cryolite. It can be obtained in solid pieces several inches in diameter, and the process of alteration can be readily followed from the perfectly fresh material. It occurs in massive aggregates of crystalline individuals which, as shown by the continuous cleavage surfaces, are often two to three inches in diameter, and are never very small. The freshest substance has usually a delicate pink or even decidedly rose color; less frequently a faint greenish tinge, and none so far obtained has the snowy whiteness or the clearness of the Greenland cryolite. The color disappears on heating, leaving the cryolite pure white. Cleavage, in three directions, very nearly at right angles to each other, is quite distinct, but although as plainly marked as in the Greenland mineral, actual separation parallel to the cleavage planes is not so easily effected as is normal. The cause of this lies, undoubtedly, in the complicated polysynthetic twin structure revealed by the microscope.

No crystals of cryolite have been found, and a thoroughly satisfactory study of the laws of twinning which appear in this massive material would require much more time than we have been able to devote to it. In some of the thin sections which have been prepared the relations are simple and admit of perfectly satisfactory explanation. Thus, in some sections parallel to the plane of most perfect cleavage, a lamellar polysynthetic twin structure like that of common plagioclase is visible. These lamellæ lie parallel to one of the two visible systems of cleavage lines, are continuous, approximately of equal width, appear most distinctly when parallel to the principal section of one of the crossed Nicols, and extinction takes place simultaneously, so far as can be determined by the weak polarization of the mineral, at an angle of about 45° from the twinning line and parallel to the diagonals of the prism as indicated by the cleavage fissures. This structure evidently indicates the law of twinning frequently noticed in the Greenland cryolite, namely, that parallel to *I* (110). The same twinning law is seen in many sections parallel to one of the less perfect or prismatic cleavage surfaces. A similar laminated structure appears, and extinction takes place at 30° to 33° from the twinning line, in opposed directions in alternate laminæ, 31° 15′ being the angle of extinction with the edge of *I*, theoretically required in sections parallel to a prism face (Groth). Such a section usually shows two systems of laminæ situated nearly at right angles to each other but seldom, if ever, crossing. The second system seems probably to represent a twinning parallel to the base. Associated with these two systems is frequently a third crossing the other two at nearly 45°, as seen in a section parallel to *I*. This may be caused by twinning parallel to a hemiorthodome, but, as stated above, the laws here represented have not as yet been fully investigated.

Chemical investigation.—For all analyses of this and the following minerals the greatest care was taken to have the purest of reagents. The fluorine was in all cases determined by the Wöhler-Fresenius method, with the slight modifications introduced by Brandl,* except that the iron plate, instead of the oil bath, was used for heating. The sulphuric acid was obtained of the highest degree of concentration and purity by distillation from a platinum retort. The water was determined by absorption in a chloride of calcium tube, the mineral having been heated in a tube with either oxide of lead or carbonate of sodium, the results being the same whether one or the other was used.

The cryolite, of which the analysis is here given, possessed the specific gravity 2·972 at 24° C., was faintly pink in color, and contained as a visible impurity the oxide of iron represented in the analysis:

Fe_2O_3	0·40
Al	12·90
Ca	0·28
Na	32·40
H_2O	0·30
F	53·55†
	99·83

The presence of a slight amount of water indicates incipient alteration. Purer material was subsequently obtained but a second analysis seemed unnecessary.

Alteration of cryolite.—The alteration of the cryolite proceeds in two ways, producing the same minerals in the end. By one process the principal cleavage fissures are utilized by the solutions which effect the change, and thin walls are formed of a white crystalline substance. The next step seems to be the bodily removal of the cryolite matter between these walls, leaving a network of partitions in the three directions of the chief cleavages of the original cryolite. These partitions or walls are lined by minute crystals. The second mode of alteration proceeds from the neighboring quartz, and from the boundaries of the different crystalline individuals of the cryolite, the result being a compact crystalline mass of a faint bluish tinge. The material at hand illustrates the two processes and their products about equally well, and they are often naturally combined.

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* Annalen der Chemie, ccxiii, p. 1.

† As the mean of 53·35, 53·46. 53·55 and 53·85.

PACHNOLITE.

a. From the thin walls.—The microscopical examination of the walls and membranes produced by the first mode of alteration of the cryolite shows them to be coated by many minute but perfect, colorless and transparent, prismatic crystals which are usually placed at right angles to the central plane of the wall, though sometimes in irregular manner. They reach a maximum length of 2^{mm} by a thickness of 0·2 to $0{\cdot}4^{mm}$. The crystals are occasionally yellow in color, while retaining their transparency, the color being doubtless owing to some matter produced by the alteration of the astrophyllite which penetrates all such specimens. The crystallographical identification of these crystals with pachnolite is quite certain, for upon placing them in vertical position under the microscope the prism angles can readily be measured and correspond closely to 81° 24′ and 98° 36′, the theoretical angles of pachnolite (see Groth, l. c. p. 463). The prism $I(110)$ and base $O(001)$ are in all cases the chief faces, accompanied frequently by a hemiorthodome, and very rarely by a clinodome, both very slightly developed. The former is considered to be $-\bar{\imath}(101)$ from data given below. Pyramid faces have not been seen upon crystals of this growth. Although all detached crystals show when examined in polarized light as lying upon a prism face, an oblique twinning plane in the prismatic zone,* still a projecting angle upon the base can but rarely be seen. The reflecting surfaces of the thinnest walls are composed of innumerable small facets of rhombic outline—the basal planes of the very low prisms. In fragments from some of the thinnest walls, placed horizontally under the microscope and observed in polarized light, twinning parallel to the shorter (ortho-) diagonal could easily be seen. The crystals are usually quite equally bisected by the twinning line. The central portion of these walls is rather dull white and probably represents the alteration product on cleavage planes of the original cryolite, while the crystals themselves were formed during or after the removal of the intermediate cryolite substance.

Thomsenolite is but rarely present with pachnolite on these walls. The few crystals found correspond to the pachnolite in size and possess a prism angle of nearly 90°, and a very perfect cleavage parallel to the base.

Material from a network of thin walls covered by pachnolite crystals was subjected to chemical analysis, yielding the result under II. p. 281.

b. From the bluish alteration product.—The pale bluish mass formed by the second mode of decomposition has in great part a regular crystalline structure produced by a more or less perfect intergrowth of pachnolite individuals in three directions approximately at right angles to each other. Nearly simultaneous reflection over the greater part of certain irregular surfaces makes this relation plain. By the examination of such a surface with a loupe, one can usually identify a number of rhombic facets which are nearly or quite coincident in position with striated planes, the two corresponding to *O* and *I* of different individuals. Such a structure is also illustrated by the crystals in the numerous small cavities occurring in the massive material.

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* Groth, l. c. p. 464.

These cavities are of wholly irregular shape and reach a maximum observed diameter of 3 to 4^{cm}. The crystals lining them are often very perfect and are occasionally 2 or 3^{mm} in length, with a thickness of 1^{mm} or less. The study of these crystals proves that, as in the preceding case, most of them must be referred to pachnolite, although thomsenolite is sparingly present. These pachnolite crystals differ in habit from those already described in that the pyramid is usually prominent, being, however, in nearly every case truncated by the basal plane; in fact, crystals without *O* are very rare. The rhombic section of the prism is everywhere plain. Although every prism on being optically tested showed a twinning plane in the prismatic zone, the low projecting angle of 179° 20′ upon *O* could seldom be distinctly seen. Many crystals are somewhat extended parallel to one pair of prism faces. A hemiorthodome of the same order as the pyramid is sometimes developed and probably corresponds to that noticed upon the crystals of the foregoing type. Most of the crystals of these cavities, while very perfect and distinctly recognizable as pachnolite, are too small and their surfaces are too frequently covered by minute crystals of a later growth to be available for measurements with the goniometer.

Whether thomsenolite is mixed with pachnolite in the massive portion or not is difficult to determine. It is certainly in comparatively small quantity if present, and in the cavities all the recognizable crystals are deposited upon the pachnolite and are apparently distinctly later in formation.

Two specimens of the bluish, massive material when carefully examined proved to contain pachnolite in a form allowing of more exact crystallographic, optical and chemical investigation.

The first of these, which we will designate specimen A, is about $8 \times 5 \times 2^{cm}$ in size, is somewhat more coarsely granular than the variety described, and possesses in an eminent degree the regular structure there observed. While quite compact in the greater part of the specimen there are portions in which the grains are more loosely aggregated together and parts of various individuals have perfectly developed crystal faces. In some minute cavities a few crystals with quite perfect terminations were found, and upon these some faces were sufficiently large and polished to admit of measurements with a Fuess reflection goniometer. These crystals are about 1^{mm} long and nearly the same in thickness. They show I (110), O (001), with subordinate -1 (111) and a negative pyramid determined as $-3\cdot\bar{3}$ (311). They are all twinned parallel to $i\text{-}\bar{i}$ (100) and the low projecting angle upon O is sometimes distinctly visible.

The angles given in the following table are all means of numerous closely agreeing measurements, and demonstrate the crystallographical identity of the mineral under discussion with pachnolite:

Angle.	Crystal *a*.	Crystal *b*.	Other Crystals.	Calculated.
$I \wedge I$ ($110 \wedge 1\bar{1}0$)	81° 19′	81° 22′	81° 18′	81° 24′
$I \wedge O$ ($110 \wedge 001$)........		90° 21′	90° 21′	90° 20′
$-1 \wedge O$ ($111 \wedge 001$		116° 39′	116° 30′	116° 30′
$-3\cdot\bar{3} \wedge -3\cdot\bar{3}$ ($311 \wedge 3\bar{1}1$.	138° 45′		138° 45′	138° 52′ 14″
$-3\cdot\bar{3} \wedge I$ ($311 \wedge 110$).....	149° 03′			149° 7′ 19″
$O \wedge O$ ($001 \wedge 001$) twin ..		179° 21′		179° 20′

The face $-3\cdot\bar{3}$ was observed on a number of distinct twin crystals with projecting angle on *O*, and in several cases on both of the negative angles.

Upon one side of specimen A are a few thomsenolite crystals, distinguishable by their prism angle of nearly 90°. They lie irregularly and seem to be later in formation than the pachnolite. Upon them are deposited minute prosopite crystals and indistinct alteration products. In the mass of this specimen no thomsenolite can be detected, while all individuals with partially free development are plainly pachnolite. Analysis III of table (page 281), was made on material obtained from the clear, loosely granular portions of specimen A, and although it was necessary to include many transparent grains of irregular shape in order to obtain a desirable amount of substance, there is no doubt in our own minds but that thomsenolite was wholly absent from the material analyzed.

The second specimen (B), from which especially good material was obtained, had a seam 2^{cm} thick of coarse granular structure running through it, and upon splitting it open along this seam two surfaces of water-clear, loosely adhering crystalline grains of pachnolite were exposed, with the regular arrangement described. Actual development of crystal faces other than the prism is rarer than in specimen A, but the size of the grains, reaching 5^{mm} in length by 1–3^{mm} in thickness, is such as to admit of the preparation of thin sections for optical examination, and also gave absolutely pure material for chemical nalysis. None of the crystals upon which the faces were well)rmed were superior to those from specimen A and only ıeasurements of the prism angles were made. The face $-3\cdot\bar{3}$ 'as not observed at all.

The optical properties of these pachnolite crystals are such as) leave no room for doubt concerning their crystallographical lentity with the mineral described by Professor Groth as achnolite. Several sections were prepared as nearly parallel) the clinopinacoid as possible. These exhibit in all cases a win structure, and this is frequently polysynthetic. The winning lines are straight and lie parallel to the vertical axis. xtinction takes place at 21° 30′—22° or 68°—68° 30′ from the winning line, in opposed directions in alternate laminæ. Ac- ording to Professor Groth the bisectrix is in the plane of sym- ıetry and inclined 68° 5′ forward from the vertical axis. ections parallel to the base show the twinning structure also, ıe line lying parallel to the orthodiagonal.

A large part of the purest crystals and ~~[illegible]~~ fragments from this ›ecimen were used for chemical analysis and repeated water :terminations, the results of which are given below (IV, ge 281).

Chemical investigation.—Previous to the analysis by J. 'andl[1] of pachnolite carefully selected by Professor Groth, chnolite and thomsenolite were considered to possess the me chemical composition. The results of all earlier analyses, cluding such as referred to manifestly very impure material, ile frequently deviating materially from the figures required theory for the formula NaF, CaF_2, AlF_3, H_2O, still agree on e whole very well, as shown in the accompanying table, and ly justified the belief in the chemical identity of the two ecies:

	THOMSENOLITE.		PACHNOLITE.					
	Wöhler.[2]	König.[3]	Knop.[4]	König.[3]	vom Rath.[5]		Hage-mann.[6]	Calculated for NaF, CaF_2, AlF_3, H_2O.
Al....	13·43	13·74	13·14	12·50	13·46	12·93	10·37	12·32
Ca....	17·84	16·79	17·25	18·17	18·10	17·99	17·44	17·98
Na ...	10·75	10·10	12·16[7]	10·23	10·68	12·06	12.04	10·34
H_2O ..	8·20	9·00	9·60	8·19			8·63	8·10
F	49·78	50·37	50·79	51·54			51·15	51·26
	100·00	100·00	102·94	100·63			99.63	100·00

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[1] Ann. d. Chem., ccxiii, p. 6.

Neues Jahrbuch für Min., etc., 1876, p. 851.

[3] Proceedings Acad. Sci. of Philad., 1876, p. 42.

[4] Annalen der Chemie und Pharmacie, cxxvii, p. 61.

[5] Sitzungsbericht d. niederrhein Ges. für Natur- und Heilkunde, 1860, xx, p. 141.

[6] This Journal, 1866, II, xli, p. 119.

[7] Also 10·80 and 10·81.

Wöhler's analysis was entirely confirmed some years subsequently by Jannasch* in Gottingen, who subjected to analysis pure thomsenolite selected by Professor Klein.

In view of the above, the analysis of pachnolite by Brandl, showing results agreeing well with those required by the formula NaF, CaF_2, AlF_3 was calculated to cause no little surprise. Professor Groth accepts without question the anhydrous nature of pachnolite and endeavors to explain away the opposing evidence shown in the foregoing table by assuming that the supposed homogeneous material analyzed was contaminated largely with thomsenolite. As in no published analysis does the percentage of water fall below 7 per cent, this assumption necessitates a most improbable admixture of thomsenolite. In those cases where the percentage of water equals or exceeds that required for thomsenolite, the presence of gearksutite is suggested by Professor Groth as a possible explanation. Even on this supposition the percentage of foreign admixture could not fall below 50 per cent, in which extreme case the whole of the impurity must be gearksutite, an amount which it is difficult to conceive should have escaped the notice of such observers as König, vom Rath and Knop, the latter of whom expressly says his analysis was made upon material identified as pachnolite.† Notwithstanding the difficulty of explaining the agreement between the previous analyses of pachnolite and thomsenolite on the assumption of the anhydrous nature of the former, the correctness of Brandl's analysis was not at first questioned by us.

In the course of the present investigations, the compact bluish material (see p. 276) having the specific gravity 2·980 at 22½° C. was first analyzed, the crystallized pachnolite not having yet been observed. The results of analysis as given under I below, agreed in the main so well with the figures required for the formula NaF, CaF_2, AlF_3, H_2O, that no hesitation was felt in considering the mineral to be thomsenolite, probably slightly contaminated with fluorite. Later, the crystalline coating on the thin walls produced by the first mode of alteration of the cryolite, the crystals forming which had not yet been identified crystallographically as pachnolite, were subjected to analysis with the results given under II. Here again the identity with thomsenolite seemed clear. It was not until the analysis of perfectly transparent fresh crystals, and crystal fragments, all taken from specimen A above described gave the results shown under III, that the possibility of the first analyses having been also made upon pachnolite, was suggested. That this was, however, so in the case of No. II subequent careful examination fully revealed, though the crystals nalyzed were not entirely free from foreign admixture. It eems certain also that the compact bluish portions (I) consist lmost entirely of pachnolite, although it cannot be positively sserted that some thomsenolite may not be intergrown with it. That no possible doubt might exist in the mind of any one as o the homogeneity of the material used for analysis III, a urther analysis was made upon crystals from specimen B above described, particular care being taken to identify each as pachnolite by the rhombic section. The results of this analysis appear under IV.

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* Neues Jahrbuch für Min., etc., 1877, p. 808.

† Neues Yahrbuch für Min., etc., 1876, p. 850.

	Hillebrand.								Brandl.†	Calculated for NaF, CaF_2, AlF_3, H_2O.
	I.		II.		III.		IV.			
l	12·02		13·02	13·01	12·23		12·36		13·60	12·32
a	19·32		15·27	15·17	18·06		18·04		18·83	17·98
g	0·13		1·53							
a	10·43		10·28		10·23		10·25		11·73	10·34
a			0·13							
$_2$O	7.87	7·95	8·64	8·79	8·10	8·11	8·05			8·10
					51·33	51·28	51·30*		55·69	51·26
					99·95		100·00		99·85	100·00

Further determinations of water on material from specimen B gave 7·95; 7·99; 8·14, and 8·15 per cent. Still other determinations, some on material every fragment of which showed he rhombic section, others on material taken at random from he crystalline mass gave results between 7·90 and 8·20 per cent. The spec. grav., at 17° C. of the perfectly pure material, as the mean of four determinations varying between 2·963 and 2·968, was 2·965. A single determination on another portion equally pure at 22° C. gave 2·962. The transparent crystals, as well as all the other portions analyzed, decrepitated violently on heating in a test tube, the walls became lined with the white deposit so characteristic of thomsenolite and pachnolite, and much water was given off. Hence, it appears that the pachnolite from Pike's Peak and thomsenolite are identical in chemical composition, unless the fact of all analyses of thomsenolite showing slightly more water than required for the formula NaF, CaF_2, AlF_3, H_2O may indicate, as suggested by Professor Groth, a partial replacement of fluorine by hydroxyl in that mineral. Should this prove to be the case, a plausible explanation of the difference in crystallization of the two minerals is offered without recourse to the theory of dimorphism.

A satisfactory explanation of Brandl's results so opposed to those presented by all earlier analyses and the ones above given is impossible, but it may be well to call attention to the fact that Brandl was obliged to make his determinations of fluorine and the metals upon quantities of 0·1106 gr. and 0·1430 gr. weight respectively, whereas, material was not wanting for the present analyses, the determinations having been made upon weights of from 0·3 gr. to 0·75 gr. It nowhere appears that a direct test for water was made upon the material furnished by Professor Groth. The latter, it is true, remarks (l. c. p. 461), "Ausserdem bildet sich bei letzterem (Thomsenolith) in den kälteren Theilen des Rohrs ein Wasserbeschlag, welcher beim Erhitzen reinen Pachnolithes natürlich ausbleibt." The absence of water does not, however, seem to be hereby proven, but simply assumed from the close approximation to 100 of Brandl's results exclusive of water. Brandl himself says water is wanting, but does not mention if this was ascertained by direct experiment. The small amount of material at his disposal renders it not improbable that no direct test was made.

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* By difference. † Ann. d. Chem. ccxiii, p. 6.

Since the conclusion of the above investigations we have, through the kindness of Mr. Albert F. Damon, President of the Pennsylvania Salt Manufacturing Company, obtained specimens of Greenland cryolite and its alteration products. From one of these specimens was removed a large number of small, needle-like, pyramidally-terminated, twinned crystals with a rhombic prismatic section, showing in fact, precisely the common occurrence[*] and ordinary habit[†] of pachnolite as described by Professor Groth. These crystals, slightly yellowish in color but quite transparent, were individually examined under the microscope, all such as did not show beyond a doubt the above described habit being excluded. They were then tested in a small glass tube for water. Decrepitation ensued on heating and the walls of the tube became lined with a white powder, and also with a deposit of water in such quantity as to preclude the possibility of its having been derived from but a small portion of the material experimented upon.

Other forms of pachnolite.—In some very cellular specimens whose walls run irregularly and seemingly without reference to the cleavage of the original cryolite, are crystals of pachnolite of different habit.

One or two of these cavities show crystals corresponding in size to those upon the thin walls, but exhibiting, each and every one, a reëntering angle on the free termination. In such little crystals the basal planes are prominent, and they are bounded on the inside by a pyramid and dome, doubtless -1 (111) and $-1\text{-}\bar{\imath}$ (101). Outward there sometimes appears another pyramid (1?) though the prismatic faces themselves usually form a sharp edge with *O*. All these crystals are too small for measurement, but as the appearance described is such as would be normal for the termination by which the twin crystals are commonly attached, it seems admissible to consider the faces as *O*, −1, −1-$\bar{i}$ and probably 1. On many twins of this kind, the outer or positive angles between *I* and *O* are replaced by two faces greatly resembling those of −3·$\bar{3}$ (311) and although entirely too minute for measurement, it is probable that the form 3·$\bar{3}$ ($\bar{3}$11) is here represented. The crystals of Greenland pachnolite are always attached by the end with the reëntering angle according to Groth.

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[*] P. Groth, l. c., p. 461.

[†] Ibid, p. 462.

Secondary formations in cavities.—In many cavities arising from both modes of decomposition of the cryolite, a second series of minerals has been deposited, chiefly as a whitish, easily crumbling aggregate of minute crystalline grains, which are recognizable under the microscope as thomsenolite, pachnolite and a mineral of the isometric system. The little thomsenolite crystals are frequently doubly terminated, and show the monoclinic symmetry plainly through the more prominent development of the negative pyramid. The pachnolite crystals are short and stout, showing in most cases only *I*(110), *O*(001) and −1-$\bar{i}$ (101). A number of large pachnolite crystals were observed which, by low powers of the microscope, seemed coated with a crystalline dust, whose particles are resolved by a power of 700–800 diameters into most perfect cubic crystals, modified by the octahedron. This is also the form of the regular crystals in the granular mass above mentioned, and were it not for a fact to be considered shortly, we should not hesitate to advocate the probable identity of these crystals with ralstonite, described by Brush* as occurring in a very similar manner upon thomsenolite from Greenland. The largest of the crystals occurring as described, is less than 1^{mm} in diameter, and no pure material even for qualitative tests can be obtained.

A new fluoride.—Just at the close of our investigations, a mineral was found, occurring sparingly in a few specimens, which seems to be very different from any known species. It was found in small cavities in the massive pachnolite, as a compact irregular mass, colorless, but not perfectly clear and exhibiting but seldom traces of crystalline form. In one specimen, however, the mass of the mineral was covered by small, rounded, crystal-like projections, which seemed like crystals of the regular system. An examination with a loupe showed the absence of recognizable faces, but such particles when broken off and tested under the microscope, proved to be fully isotropic. A few faces found on one crystal seem to belong to cube and octahedron, and particles detached from the same are isotropic in action in polarized light. Supposing that this substance must be ralstonite in exceptional development, enough material for the following partial chemical analysis was selected, being carefully freed from attached particles of pachnolite and other anisotropic minerals, by microscopical examination. The Al, Ca and Mg were accurately determined, the Ka and Na, owing to an unfortunate mishap, only approximately. No water could be detected by direct test upon a small portion. Fluorine was present in quantity, and the percentage given below is calculated on the assumption that the metals are fully combined with it.

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* G. J. Brush, this Journal, III, ii, p. 30, 1871; also P. Groth, l. c., p. 471.

Al	11·40
Ca	0·72
Mg	0·22
Ka	28·94
Na	9·90
F	46·98
	98·16

From the above data may be derived a formula analogous to that of cryolite, in which about two-thirds of the sodium is replaced by potassium. Further investigation will be made upon this most interesting mineral as soon as better material can be obtained. The presence of this regular mineral, naturally renders it impossible to refer the tiny crystals observed upon pachnolite to ralstonite with any degree of certainty, but we are inclined to believe that the two minerals are not identical.

GEARKSUTITE.

This mineral, first observed by Hagemann* on compact thomsenolite, and described as earthy and kaolin-like in aspect, dull, white, opaque and of hardness 2, seems to be so rare in connection with the Greenland fluorides, that no one has had material for further examination. Groth† found it in very small quantity among the minerals at his disposal, but could not obtain enough for analysis. He, however, found that it consisted of very minute microscopic needles, with oblique extinction, and considers it as undoubtedly a definite species.

Among the minerals from St. Peter's Dome, gearksutite is quite abundant. It is not formed from other minerals by molecular replacement, but is deposited from solution, in the cavities upon fresh crystals of pachnolite, etc. Smaller cavities are sometimes filled by it, and on the contact with the quartz it is specially developed.

In appearance, it corresponds closely to the description of the Greenland mineral, as given by Dana, the resemblance to the purest, finest kaolin being especially remarkable. Examined microscopically, gearksutite is seen to consist, as stated by Prof. Groth, of exceedingly minute colorless needles, the average length of which is less than 0·02^{mm}, and the thickness less than 0·002^{mm}, and apparently possessing oblique extinction.

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* Dana. System of Mineralogy, 5th Ed., p. 130. † Groth, l. c., pp. 460 and 481.

Chemical investigation.—Gearksutite was found by Hagemann (l. c.), to possess the following composition:

Al	15·52
Ca	19·25
Na	2·46
H_2O	20·22
F	41·18
	98·63

An examination of the above results shows that the atomic ratio of $Al : Ca+Na_2 : Fl$ is $1 : 1 : 4$, instead of $1 : 1 : 5$ which would represent complete saturation and require about 12 per cent more fluorine than was found. On the assumption that the missing fluorine is replaced in the mineral by oxygen or hydroxyl, there should appear a much greater loss than the analysis indicates. An error is therefore evident, probably in connection with the determination of the fluorine, or water, or both, in consequence of which the construction of a satisfactory formula has heretofore been impossible.

The material for the following analyses was first partially crushed, then freed from admixed heavier particles of foreign matter by triturating in a beaker with water, the impurities falling to the bottom of the vessel, while the light gearksutite remained suspended in the liquid and was removed by decantation. By repeating this operation a great many times, a product was finally obtained entirely free from all foreign admixture. It was allowed to settle completely, the supernatant liquid poured off and the residue dried first on the water bath, then at 100° C. Thorough pulverization of this residue is a difficult matter, as it flattens out under the pestle, forming flakes which strongly resist the pulverizing action. This is of little moment, however, since the flakes are so spongy as to offer no hindrance to attack by sulphuric acid. Two analyses were made from the same sample, with the results tabulated below. In *b*, sodium and potassium were not determined.

	a.	*b.*	Mean.
Al	15·33	15·30	15·31
Ca	22·29	22·32	22·30
Na	0·10	----	0·10
Ka	0·04	----	0·04
H_2O	15·54	15·39	15·46
F	42·14	42·01	42·07
	95·44	----	95·28
Loss as oxygen	4·56	----	4·72
	100·00		100·00

Taking the figures in the third column and combining the fluorine with the calcium, sodium, potassium, and, as far as possible with the aluminium, there remains of the latter 5·32 per cent, requiring 4·66 per cent of [illegible] oxygen, an amount agreeing very closely with that obtained above by difference and making the sum total almost exactly 100.

22·30	Ca	requires	21·18 F.
0·10	Na	"	0·08
0·04	Ka	"	0·02
9·99	Al	"	20·79
9·98	Al_2O_3	"	42·07
15·46	H_2O		
42·07	F		
99·94			

The agreement of the above analysis with that of Hagemann, after substituting in the latter for the sodium its equivalent in calcium, is very close, with the single exception of the water. As his analysis was manifestly erroneous in some particular, the assumption of the identity of gearksutite with the mineral here discussed is fully justified, supported as it is by the similarity in occurrence, appearance and physical characteristics. His error would then consist in having obtained from four to five per cent too much water, a result not difficult of explanation, in the case of a hydrated fluoride, if no precaution was taken to prevent the escape of fluorine.

Substituting in the mean of analyses *a* and *b* for sodium and potassium their equivalent of calcium, and dividing the percentages by the atomic weights, the atomic ratio is found to be as given below:

Al $15·31 \div 27·4 = 0·559$
Ca $22·41 \div 40 = 0·560$
H_2O $15·46 \div 18 = 0·859$
O $4·66 \div 16 = 0·291$
F $42·07 \div 19 = 2·214$

The ratio of Al : Ca : F is here nearly as 1 : 1 : 4, the same as found by Hagemann. Subtracting from the atomic value for water, an amount 0·291 equal to that for oxygen, in order to form with the latter hydroxyl, the result is as given under I, while under II appears the ratio referred to calcium as unity.

	I.	II.	
Al	0·559	1·00	
Ca	0·560	1·00	
H_2O	0·568	1·01	
HO	0·582	1·04	4·99
F	2·214	3·95	

It will be seen that by combining hydroxyl and fluorine the ratio Al : Ca : H_2O : (F, OH) is 1 : 1 : 1 : 5 and the formula for the mineral becomes CaF_2, Al(F, OH)$_3$, H_2O, in which the fluorine and hydroxyl combined with the aluminium stand nearly in the proportion 2 : 1. Were the latter proportion exactly fulfilled, the formula might be written $3CaF_2$, $2AlF_3$, $Al(OH)_3$, $3H_2O$ requiring the percentages:

Al 15·36, Ca 22·42, F 42·60, O 4·49, H_2O 15·13 = 100·00

Of the 15·46 per cent of water found by analysis, 5·24 per cent has been considered in the foregoing as basic. While this amount may, on theoretical grounds alone, enter into the nner constitution of the mineral as basic water, the remainder cannot, but must be water of crystallization. That a portion of the water is basic, is rendered more than probable by the fact that at 300° C. some is still retained. In this connection, the following experiments were made. 0·5677 gr. of the mineral, not however from the same sample as that used for analysis, dried first at 100° C. and contained in a platinum crucible, was exposed in an air bath during 145 hours to temperatures ranging from 100° C. to 300° C., the weight being taken at intervals averaging ten hours each. The results in brief showed that at 145° C., the loss was but 0·35 per cent, at 230° C. only 0·92 per cent, at 250° C. 7·02 per cent, after prolonged heating at 265–270° C. 9·49 per cent, and at 295° C. 13·92 per cent. As no further loss occurred after six hours' heating at 295–300° C., a portion of the residue, which still retained its original appearance, was subjected to a quantitative test for water, of which 1·76 per cent was found. This added to the 13·92 per cent driven off below 300° C. made the total 15·68 per cent. Since this is slightly higher than the mean of the previous results, it seemed possible that some fluorine might have escaped. The remainder of the residue was therefore tested quantitatively for fluorine, of which was found 40·60 per cent, thus proving the correctness of the surmise. A similar experiment with the same general results was made upon a smaller portion of another sample. A comparison of the full results of both experiments showed that by sufficiently prolonged heating at approximately 270° C., all the water of crystallization could be driven off, also that by still further heating at little, if any, higher temperature, the basic water began to escape, but was not entirely expelled, even after many hours' exposure to a temperature of 295–300° C.

In the Journal of the Chemical Society for 1883, page 140, Walter Flight describes a mineral obtained from the cryolite bed of Greenland. "It is made up of a congeries of minute white transparent crystals, mostly broken up and lying entangled among each other in every sort of direction, which gives the mass an appearance of opacity much resembling that of kaolin or chalk."

Chemical analysis showed it to consist of

						Equivalents.
Al	16·23	with F	33·64	=	49·87	0·59
Ca	22·39	" "	21·27	=	43·66	1·12
Na	0·43	" "	0·33	=	0·76	
					94·29	
			Water		5·71	0·63
					100·00	

The fluorine seems to have been calculated for the metals, and the water was found by difference.

From the above data the author obtains the formula $2CaF_2$, Al_2F_6, $2H_2O$,* and considering the mineral new, names it *evigtokite.* Mr. Flight seems to have overlooked the description of gearksutite in Dana's System of Mineralogy, and Prof. Groth's remarks upon the same (l. c. pp. 481 and 493), else the very fair agreement of his analytical data for the metals with those of Hagemann, and the similarity in occurrence, appearance and physical characteristics of the two minerals must have led to at least a suspicion of their identity. There can hardly exist a doubt that Flight has analyzed gearksutite, and that the name evigtokite is therefore to be dropped.

PROSOPITE.

This rare species hitherto unobserved in association with the cryolite minerals, and known only in connection with the tin-bearing veins of Altenberg, in Saxony, has been identified in both veins at St. Peter's Dome. It is most abundant in vein B, and the chief description of it will come under that head, but close examination has proved its presence also among the minerals of vein A.

Both of the coarsely crystalline specimens of pachnolite, above described as A and B, have prosopite upon them. Specimen B is, in parts, in process of alteration to a dull, white, porous substance, with little cavities in which are minute crystals of prosopite. These are colorless, transparent, tabular in shape, showing *i-í* (010) predominant, *I* (110), 1 ($\bar{1}11$) and $-2\cdot\bar{2}$ (211), agreeing exactly in form and optical behavior with those determined as prosopite in vein B. In two other specimens of pachnolite, prosopite tablets may be seen upon certain granular surfaces when decomposition of the pachnolite has already begun. The crystals are usually attached by the prismatic edges, although free and perfect terminations are to be found. Gearksutite was noticed upon them in one specimen, and the position of prosopite in the series of hydrous fluorides is doubtless between thomsenolite and gearksutite. No material for chemical tests could be procured from the specimens described.

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* Probably a printer's error. It should read $2CaF_2$, Al_2F_6, H_2O.

The penetration of the fluoride mass by astrophyllite blades springing from the side of the vein has been mentioned (p. 273). Here the cryolite has been altered to cellular pachnolite, according to the first mode described, and this has for the greater part given way to other products or been dissolved and carried away, leaving the astrophyllite blades more or less free or imbedded in gearksutite and other soft, crumbling material. The free blades usually have a coating composed of a little purplish fluorite immediately adjacent to the blades, and over this a nearly colorless or slightly yellowish substance. At and toward the base of the astrophyllite blades the latter is present in roundish aggregates made up, as shown by the loupe, of clear tablets, in more or less radiate arrangement. The crystal form is here quite obscure, but the general appearance was so suggestive of prosopite that by sacrificing the best specimen enough material was obtained for determination of the bases and of fluorine, with the result given below—V, p. 293.

VEIN B.

Description.—The minerals found in this vein have been exposed by the "Eureka" tunnel which has been driven in upon it for one hundred feet or more. Unfortunately the walls are so blackened by the smoke from the blasting that the relations of the different mineral bodies encountered to each other cannot be clearly seen, and the data here given are derived from the study of the material upon the dump and from a partial examination of the tunnel walls with the aid of a hammer.

The main part of the vein is white, massive quartz with here and there immense individuals of pinkish feldspar, which proves on microscopical examination to be microcline with thin, irregular laminæ of albite intergrown nearly parallel to the macropinacoid. This microcline is sometimes wholly altered to pure white, coarsely foliate kaolin, which is impregnated with fine particles of galena.

Smaller and apparently wholly irregular parts of the vein are occupied on the one hand by quartz containing zircon in great abundance, quite equally distributed through it, and on the other by a mass of mixed fluorides. The quartz containing zircon (see this Journal, October, 1882, page 284), is always sharply defined from the massive white quartz of the main vein which is wholly without included minerals. The zircon crystals are imbedded largely in the quartz itself and to a less degree in compact, white kaolinite, a greenish yellow mica, and purple or green fluorite. These three minerals fill small irregular spaces in the quartz, and probably replace feldspar, for they are also found on fissure planes as distinct secondary deposits.

The kaolinite is usually very compact, but crystals were found in some small cavities. These are exceedingly thin, colorless, transparent, rhombic leaves, the acute angle being usually evenly truncated, producing sometimes an almost perfect hexagon. The result of many measurements under the microscope seems to indicate that the obtuse angle of the rhomb is slightly less than 120°, the best results varying from 118° 30′, to 119° 30′. The thinnest leaves show distinct action on polarized light, and extinguish parallel to the diagonals of the rhomb. The thicker crystals are made up of many thin ones which are usually not perfectly coincident in position, and sometimes form more or less perfect rosettes.

The mica occurs in masses or foliated upon fissure planes, and no crystals have been found. The kaolinite possesses the composition I, and the mica II, as given below. The kaolinite contained a small amount of fluorite in almost microscopic crystals, the quantity being calculated from the Ca found.

I.	
SiO_2	45·93
Al_2O_3	39·65
H_2O	13·77
CaF_2	0·84
	100·19

II.	
SiO_2	52·59
Al_2O_3	29·72
Fe_2O_3	1·40
CaO	0·26
MgO	2·12
K_2O	8·33
Na_2O	0·50
H_2O	4·39
	99·31*

The fluorides seem to fill the small irregular spaces left after the de[illegible]position of the zircon-bearing quartz, the thickness of the fluoride mass varying from a thin seam to possibly two or three feet. They are always sharply defined against both pure and zircon-bearing quartz, the contact surfaces of the former being sometimes apparently huge crystals.

The greater part of the fluoride mass is now dull white and very compact, and is evidently made up of a mixture of at least two substances, neither of which has as yet been identified. Only in a single specimen is there any clue to the original mineral, but in this there is a small, solid mass of unmistakable cryolite with alteration to pachnolite(?) progressing upon its cleavage planes in the exact manner described in vein A, but passing into the compact white mixture already mentioned.

Adjoining the quartz is usually an irregular zone of purplish or greenish fluorite, and next to this a rather coarsely granular mass of a colorless mineral with two distinct cleavage planes which passes gradually into the compact white substance. This zonal arrangement is not without exceptions, for both granular and compact masses come directly in contact with the quartz in some specimens, and fluorite is more or less abundantly sprinkled through the other substances; in fact the relation of the minerals to each other is such as to indicate that they are but different phases of alteration from a common source. The granular mineral occurs in sufficient purity to afford material for chemical analysis, and its individuals are large enough to admit of the preparation of thin sections with definite relations to the cleavage planes. The analysis first proved the identity of this mineral with prosopite, the optical properties shown by the thin section confirmed this determination and quite recently a few minute crystals were found in one specimen which agree with the published data on the Saxon mineral. As the identification of this rare species, particularly in its present association, is a matter of considerable interest, we will describe it somewhat in detail.

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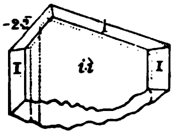
* The presence or absence of fluorine was not ascertained.

PROSOPITE.

Crystalline form and physical properties.—The minute crystals found in a single specimen from vein B as well as those bserved on pachnolite, etc., in vein A p. 288) are all of the habit shown in the ccompanying figure. This is from a camera ucida drawing of a crystal measuring 0·5^{mm} ormal to the edge of the prism, and can herefore make no pretensions to crystalographical accuracy. The crystals are colorless and transarent, have uniformly a tabular form through the developnent of *i-ī* (010) and show plainly the prism and two pyramids which may be considered as 1 ($\bar{1}11$) and $-2\cdot\bar{2}$ (211) for xtinction takes place nearly or quite parallel to the edge f $-2\cdot\bar{2}$, which is, according to DesCloizeaux* and Groth,† the osition of the bisectrix.



Thin sections prepared as nearly as possible perpendicular to he edge of the two cleavage faces in the irregular granular ndividuals, show that the angle of the cleavage planes is very early 135°, and that extinction takes place parallel to the irection bisecting that angle. This behavior agrees perfectly ith the statements concerning prosopite, according to which he chief cleavage is parallel to $-2\cdot\bar{2}$, the angle of which is bout 134°. The present material does not allow of a definite ettlement of the question of the crystalline form of prosopite, but nothing observed is in conflict with the reference to monoclinic system.

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* Bull. Soc. Min. de Fr., v, 317.

† l. c., p. 290.

Chemical investigation.—The formula deduced by Bra for prosopite, from the results of his analysis as here give

		Atomic values.
F	35·01	1·842
Al	23·37	0·853
Ca	16·19	0·405
Mg	0·11	0·004
Na	0·33	0·014
H_2O	12·41	0·689
Loss as oxygen,	12·58	0·786
	100·00	

is CaF_2, $2Al(F_2,OH)_3$. The whole of the water is assum be basic, entering with oxygen into the constitution o mineral as hydroxyl, the latter replacing an equivalent am of fluorine. In support of this assumption, Brandl men that no loss is perceptible below 260° C.

In an earlier partial analysis, Scheerer (Pogg. Ann., 361) found

Al 22·77, Ca 16·41, H_2O 15·50

Of the analyses tabulated below, those under I, II, II IV were made upon material from vein B. That anal under I, *a* and *b*, was composed of comparatively large ir lar crystalline pieces, showing no visible impurity what having a sp. grav. at 23° C. of 2·880 and a hardness of 4·5. As the ratio Al : Ca differed materially from that o required by Brandl's formula, it appeared that some fo matter must be present, consequently no further determina were made, as it was hoped better material might be obta

Analysis II was made upon material separated from q zircon, fluorite and other accompanying minerals by a sol of iodide of mercury in iodide of potassium. The result a slight improvement upon the previous and the analysi completed. The material for III was picked out care by hand with the aid of the loupe, but as the result was not satisfactory, a further portion (IV) aggregating how only 0·1022 gr. was selected with the greatest possible every particle being distinctly crystalline and showing u the microscope no trace of impurity. Here a slight imp ment becomes evident in the ratio, but as the amount take analysis was so extremely small, it cannot be asserted tha better results may not be due to unavoidable errors of ana The material for analysis V was derived from vein A, whe prosopite occurred on astrophyllite associated with cry pachnolite, etc. This material was, however, evidentl uite pure, being opaque and very slightly colored in part by xide of iron. The analysis was made merely to prove its dentity with the prosopite from vein B.

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* Ann. de Chem., ccxiii, p. 13.

	I.		II.	III.	IV.	V.
	a	*b*				
Al	22·09	21·83	22·03	22·28	22·63	21·79
Ca	17·67	17·87	16·92	17·14	16·80	16·84
Mg	....	....	0·20	....	0·15	0·35
Na	....	....	0·48	....	0·48	0·79
Ka	....	....	....	....	....	0·11
H_2O	....	....	13·54 (13·37)	....	....	....
F	....	....	33·14	33.22	....	32·30
			86·31			
	Loss as oxygen,		13·69			
			100·00			

The mean of all the results under analyses I–IV is as follows:

Al	22·17
Ca	17·28
Mg	0·17
Na	0·48
H_2O	13·46
F	33·18
	86·74
Loss as oxygen,	13·26
	100·00

After subtracting from the fluorine an equivalent for the calcium, magnesium and sodium, and combining the remainder with aluminium, there remains of the latter 14·44 per cent, requiring 12·65 per cent oxygen, instead of 13·26 per cent found by difference. The atomic values appear as follows, after substituting for magnesium and sodium, an equivalent of calcium:

Al	22·17 ÷ 27·4	0·809		
Ca	17·98 ÷ 40·0	0·449		
F	33·18 ÷ 19·	1·746		3·285
H_2O	13·46 ÷ 18·	0·748	1·539	
O	12·65 ÷ 16·	0·791		

The result is unsatisfactory, the ratio of Ca : Al being as 1 : 1·78 instead of 1 : 2. In none of the material analyzed was the slightest trace of kaolinization to be observed, nor any other foreign matter. It therefore becomes impossible to explain with any degree of certainty the above abnormal results.

The general agreement of all the analyses, the aluminium being found too low and the calcium too high in each case, shows pretty conclusively that an explanation cannot be sought for in analytical errors.

As fluorite occurs here always in most intimate association with prosopite, and the possibility suggested itself that some of this might be so intergrown with the latter as to escape the closest scrutiny, it became desirable to ascertain what change would be effected in the ratio above given by subtracting enough calcium to make the ratio Ca : Al as 1 : 2 and an equivalent amount of fluorine. The atomic values then become:

Al	0·809		2·00
Ca	0·404		1·00
F	1·656	3·195	7·91
HO	1·539		

which agree quite as well for the formula $CaAl_2(F, OH)_8$ as those obtained by Brandl.

If instead of the mean of all the analyses, the figures of II alone are taken for calculations similar to the above, the result is the same, even a little more closely approximating to the ratio 2 : 1 : 8.

The observation made by Brandl, that below 260° C. no loss in weight occurs, was found to apply here, provided the exposure to this degree of temperature is short. If continued for many hours a slight but sensible loss is observed.

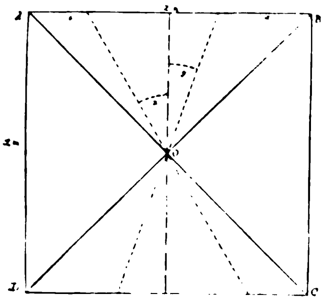
Denver, Colorado, June, 1883.

ART. XXXI.—*On the Origin and Hade of Normal Faults*; by W. J. McGEE.*

FAULTS may be either *normal* or *reversed*; i. e., may hade to either down-throw or up-throw. Both classes are probably developed by movements relieving radial strain (either simple or differential), tangential thrust, tangential tension, or a combination of these stresses, in the terrestrial crust. Only those due to radial strain in its essential combinations are here considered.

Let contiguous areas of a homogeneous rigid tract resting on a mobile sub-stratum (which may be either the gaseous, fluid or *quasi*-solid terrestrial nucleus, or a yielding layer any where within the exterior shell) be in unstable equilibrium, such as might arise from transference of sediment, and let all initial stresses save those immediately due to the disturbed equilibrium be at first supposed eliminated. In such case the tract will suffer differential radial strain culminating in the plane of coincidence of the unstable areas.

1.



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* Read before the Iowa Academy of Science, May 31st, 1883.

Now manifestly, before fracture supervene the strain will be distributed over a considerable horizontal zone, and will be propagated vertically through the tract; whence the resultant of stresses at any point in the plane of imminent faulting will bisect a parallelogram similar to that whose base and perpendicular are respectively the width of the stressed zone and the thickness of the homogeneous tract. Make such parallelogram equilateral; denote base and perpendicular by $2n$ and $2m$ respectively; and let BC be the depressed side. The strain will then be resolved into stresses of opposite sign acting in the directions of the diagonals.

If now fracture relieve tensile strain only, it will occur at right angles to the tension-diagonal AC, or in BD; and the fault will hade to the up-throw. Such indeed is the direction actually taken by ice-stream crevasses (to which the diagram will equally apply) as shown by Hopkins;* for in slowly moving ice the crushing strength must be nearly *nil* and the compressive-elasticity infinite, while tensile stress probably increases rigidity and brittleness, and thus diminishes the tensile elasticity.† And were the antithesis of ice existent its fracture under the like stresses would occur at right angles to the compression-diagonal, or in AC, and its fault would hade to the down-throw.

But the materials of faulted tracts possess both tensile and crushing strengths, and also tensile and compressive elasticities. Denoting these in their order by t, c, t' and c', the values $t\times t'$ and $c\times c'$ are obtained for the moments of fracture orthogonally to the diagonals AC and BD respectively; which values may be represented by a and b, whose sum is proportional to $2n$. Obviously, if a and b be equal, the fracture will bisect the angle of the diagonals in nO; but if a be in defect (as in crystalline solids generally) the hade will ever be to the up-throw in the angle y, whose tangent is $(n-a)m$. From this expression the hade of fault in any greatly attenuated tract of determinate stress-parallelogram and known strength and elasticity might be readily computed.

If the case be modified by the introduction of initial tangential stress, such stress will operate through mO with intensity i, and tend to produce fracture in nO. If the stress be tensile it must be less than a, and will at once relieve BD and strain AC proportionally to its intensity by the cosine of the angle formed by its direction and that of the latter diagonal, and thus (denoting such angle by θ) reduce a by $i\cos\theta n$; when the expression for hade will become $\tan y=(n-\overline{a-\overline{i\cos\theta n}})m$, and the hade will ever diverge from the normal. Conversely, if the stress be compressive the relation will become tan $y=(n-a+\overline{i\cos\theta n})m$ or tan $x=(n-b-\overline{i\cos\theta n})m$ as the case may be, and the hade will ever approach the normal; but if it become normal the tendency to movement will be counteracted and a disposition to produce dragged fault or flexure will be engendered; since the development of normal faults demands that the lateral extent of the faulted tract shall increase from T to T+(tan $h\times d$) where h is angle of hade and d the vertical displacement.

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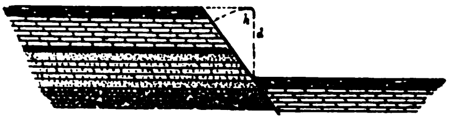
* *Trans. Camb. Phil. Soc.*, viii. (pt. ii, 1844, 163–7.)

† *Vide* 'On Maximum Synchronous Glaciation, III, 1, 2: *Proc. A. A. A. S.*, xxix, 1880, 497.

2.



(The case in which tangential thrust is predominant, and the tendency hence toward ordinary reversed faults, complex folding and tumefaction, falls beyond the scope of this discussion.)

It thus appears that the general disposition of differential radial strain, either singly or combined with tangential strains, is to form reversed faults.

Let the case be farther modified by the introduction of simple vertical stress apart from those due to the unstable equilibrium. Were the stress tensile, its effect would be analogous to that of tangential tension; but since general vertical tension without predominant tangential thrust is inconceivable (whence the sum of tensile vertical stresses is always in defect), and since local vertical tension must equally react on adjacent *couches*, the effect of such stress is nugatory. But if the stress be compressive it will equally relieve AC and strain BD, and thus diminish b; and since within the crushing strength of the material of the tract there is no limit to this stress, b will be reduced to $b-\overline{v\cos\theta m}$, where v is such vertical stress, and the original expression for hade will become tan $x=(n-b-\overline{v\cos\theta m})m$; when the fault may hade strongly to the down-throw.

The natural case favorable to profound faulting arises in such an unstable tract as has been assumed within which exist vertical stresses due to its own weight and sufficient initial tension to admit of free lateral movement as rupture occurs. In such case the strain in BD and the disposition to give way in AC will progressively increase downward until the crushing strength of the material is reached; whence *the fault due to radial strain must originate with normal hade in a deep-seated hypogeal couche, and* (since rupture in each *couche* will throw the strain on the next higher) *must be propagated upward with progressively diminishing hade*, which may eventually become nothing or even reversed; *while below the couche of origin the fracture* st *give place to plastic flexure.* The full expression for nent of fracture and angle of hade in this case is $\tan x =$ $\overline{\cdot b - v\cos\theta m + i\cos\theta n)}m$; and since the several factors may ipproximately evaluated it follows that the regimen of faults ned under given conditions, and conversely the conditions ing rise to faults of known regimen, are roughly deter- iate.

}elow the couche of origin the substance of the tract must in the condition of ice in a Brahma press or of lead in a e machine; and for the purposes of the present discussion, or the purposes of physical geology generally, it is imma- al whether the mobility be molar or molecular. Obviously h substratum would be equivalent in effect to the *quasi*- d terrestrial nucleus assumed at the outset.

f the unstable equilibrium were due to transference of sedi- nt, the sub-surface strata would be compressed beneath the led area and allowed to expand under the lightened area, l the throw, as measured by corresponding strata, would sequently increase toward the surface.

n profound faulting, such as is here considered, minor de- tures of the plane of fracture from the plane of maximum ss may be neglected, since (1) the irregularities would be itively slight, and since (2) incipient movement would tend liminate all such irregularities.

'ractures of the origin and character here contemplated may lenominated *Normal Faults of the First Order.*

et a homogeneous rigid tract be subjected to simple ver- l compressive stress due to its own weight. Within the ts of its elasticity, resulting vertical compression will be ved by lateral extension; but beyond such limits lateral ss will be in defect, and fracture may occur in each prism of stratum of imminent crushing just as if it were independ- and unsupported laterally. Now as shown by Hodgkinson,* ture in crushed prisms of crystalline texture occurs diago- y to the direction of stress and separates the prism into lge-shaped segments; whence the stratum of incipient shing may develop a series of diagonal fractures each bi- ing a prism of height equal to the thickness of the stratum of width determined by the constitution and texture of the erial fractured. Only so much movement of the pyramidal nents can occur as will serve to equalize vertical and lateral sses: the hade will ever be normal; and from the center each plane of fracture the throw will progressively increase vard and downward to the apices of the contiguous seg- nts. Such may be termed *Normal Faults of the second order.*

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* Cited by Rankine, "Civil Engineering," 4th ed., 1865, 235.

Let a homogeneous rigid tract resting on a mobile substratum, or a tract consisting of alternate rigid and yielding strata, be subjected to vertical compressive stress due to its own weight; and let the rigid portion be traversed by oblique planes of fracture or of weakness due to any cause. Each rigid pyramid, prism or wedge bounded by such planes will then seek a hydrostatic equilibrium and will sink into or be upraised by the mobile substratum to a degree determined by the area of its base and its mass, and the initial planes of fracture or weakness will become faults of normal hade. As in faults of the first order, beneath the *couche* in which pressure equals crushing strength fracture will give place to flexure; and, since the behavior of any yielding stratum will simulate that of the mobile substratum, if there be alternations of rigid and yielding strata, or if all strata be imperfectly rigid, the throw will increase toward the surface.

Dislocations of this character may be designated *Normal Faults of the Third Order.* The principles involved in their development were long ago pointed out by Hopkins,[*] and are now too generally recognized to require thorough analysis.

Except in the last case the influence of joints in determining the regimen of faults may be neglected; for joints are known to be superficial, while faults are deep-seated.

Since the movements contemplated in the second case must be confined to strata of limited thickness, while in the third case the assumption of profound initial fractures or planes of weakness is unwarranted, the grander faults of the globe must be referred to other causes; and since differential radial strain is an adequate cause, while the phenomena of profound normal faults are collectively such as differential radial strain tends to produce, the hypothesis that these faults are due to such strains is warranted, and is here enounced.

If the hypothesis be valid, it will afford the means of coördinating faults and flexures; it will at once sustain the suggestion of Gilbert[†] that Appalachian flexures and Cordilleran fractures are but diverse manifestations of identical movements, and render the amount of denudation which the former region has suffered roughly determinate; it will go far toward demonstrating that, whatever be the condition of its interior, the terrestrial crust is—as already indicated by Dutton[‡]—in hydrostatic equilibrium; and it will afford a new vantage-point from which many of the complex problems of orology and terrestrial physics may be approached.

Salt Lake City, Utah, March 23d, 1883.

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[*] "Researches in Physical Geology," *Phil. Trans. Roy. Soc.*, 1842, i. 53.

[†] "U. S. Geog. and Geol. Surveys West of the 100th Meridian," III, Geology, 1875, 62.

[‡] Notice of Fisher's "Physics of the Earth's Crust," this Journal, xxiii, 1882, 288.

ART. XXXII.—*On the Sensitiveness of the Eye to Slight Differences of Color;* by BENJAMIN OSGOOD PEIRCE, JR.

AUBERT has shown* by his experiments with revolving liscs that the eye is able to detect the change produced by the iddition of 1 part of white light to 360 parts of colored light, nd that perceptible changes in hue can be brought about by dding to light of any color less than 1 per cent of light of a lifferent color. He infers from this that a normal eye could listinguish at least one thousand different hues in the solar pectrum.

At the suggestion of Professor Wolcott Gibbs, I have made few experiments to test the sensitiveness of the eyes of different people to slight changes of wave-length in different parts of he spectrum.

For this purpose, a long, thin sheet of vulcanite was inserted engthwise into the collimator of a large spectroscope so as to livide the tube into an upper and a lower half. The lower part of the tube received light from a fixed slit, the upper part rom a movable slit of the same width, which could be set xactly over the other, or displaced to the right or to the left. The amount of displacement was determined by means of a steel scale fastened to the upper slit and moving with it past a ixed zero point. The light from the collimator fell upon a Rutherfurd diffraction-grating of about 17,000 lines to the nch, and the resulting spectra were then thrown, one above the other, into the observing telescope. A blackened, metallic diaphragm, out of which two narrow slits had been cut in the same vertical line, was placed in the eye-piece of the telescope, so that when the two collimator slits were even the observer saw merely two narrow strips (one over the other) of the same colored light on a black field. When the movable collimator slit was displaced, the *color* of the observer's lower strip was changed without changing its *position in the field*, and the object of the experiments was to see how small a displacement could be infallibly *detected* and *named in direction* by the observer.

The width of the collimator slit was about ·25^{mm}, and the slit in the eye-piece diaphragm was nearly of the apparent size of the collimator slits as seen through the telescope. The length of that part of the spectrum which was so bright as to be easily studied (say from Li α to G) extended over more than 12 degrees of arc as measured in the instrument, and the width of the strip seen by the observer was about 5 minutes of arc, so that not more than the $\frac{1}{150}$ part of this brighter part of the spectrum was in the field at the same time.

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* "Physiologie der Netzhaut," Breslau, 1865, pp. 132–154; Rood, "Modern Chromatics," pp. 39–41.

In no case was the observer able to detect any difference between the colors of the two edges of a strip; each strip seemed to be of one color throughout.

It was necessary to regulate very carefully the Argand burner used as a source of light, for the least difference in the intensities of the two spectra made the strip of the brighter seem to the observer yellower than the corresponding strip of the other spectrum. This error, due to the "color of brightness,"* was most apparent in observations made at the extremities of the spectrum, i. e. in the red and the violet.

A displacement of the movable collimator slit amounting to 1 scale division corresponded to 4 minutes of arc, or to a difference in wave-length of $\cdot 0000013^{mm}$.

The observer sat in a darkened room and directed the telescope to that part of the spectrum in which the observations were to be made. After an assistant had placed the movable collimator slit even with the other, and the observer had seen to it that under these circumstances the strips of light in the eye-piece seemed just alike—as they should—the observer took his eye away from the instrument, and the assistant displaced the movable collimator slit and asked the observer to look into the telescope and to tell him, from the colors of the strips, in which direction the slit must be moved in order to be even again with the fixed slit. If the displacement was very large, the observer was able to tell immediately how to move the slit in order to match the colors of the strips; if in successive trials the displacements were made smaller and smaller, a place was reached where the colors of the strips did not seem well matched to the observer, but where he could no longer say with confidence *in which direction* the movable slit had been displaced.

The smallest displacements which the observer could infallibly detect and name were determined at different places in the spectrum, and it soon appeared, as was to have been expected, that the magnitude of these displacements was very different with the same person for different colors and not by any means alike with different persons for the same color.

Though the results seemed at first somewhat irregular, I found that the same series of results were invariably obtained with the same person, even though the experiments were separated by days or weeks, and that certain features were common to all the series obtained with the different observers.

The largest displacement needed (except at the ends of the spectrum) by any of the observers corresponded to a difference in wave-length of the middle of the two strips of about $\cdot 000005^{mm}$, and the smallest displacement which any observer could infalibly detect corresponded to a difference in wave-length of only 000005^{mm}. This last is rather remarkable, for although no ne could see any difference between the colors of the edges of ither of the strips, a displacement which made the color of the niddle of one strip the same as that of an edge of the other ometimes made the two strips distinguishable.

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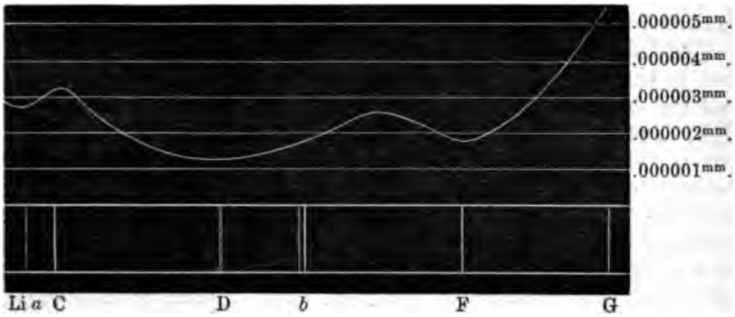
* C. S. Peirce, "Note on the Sensation of Colors," this Journal, April, 1877.

There was a singular uniformity in the performance of differnt eyes as judged by the average of these displacements for he whole visible spectrum. Perhaps smaller differences might ave been detected if the strips had been narrower, but the vork proved to be so trying to the eyes that I thought it best ot to experiment further.

From a series of observations made by a number of different ersons and extending over several months, two or three genral conclusions may be drawn. To make these evident, I have lotted a curve by laying off over different places in the specrum ordinates obtained by averaging in each case the least isplacements distinguishable by the different observers at that oint. The general features of this curve appeared in the sults of every observer.



In all cases the eye was most sensitive to changes in a color ightly less refrangible than that of the sodium line, though is color varied somewhat with different persons, being in ome cases more orange and in others more yellow. In all ases the eye was more sensitive to changes in the color corresonding to the F line than to changes in colors lying half-way etween *b* and F.

In many cases, though not in all, the eye was less sensitive changes in a red near the C line than to a somewhat darker d beyond the Lithium line. In the darker colors at the ends f the spectrum it was, of course, very hard to distinguish small ifferences.

In addition to these general features there were in most of he curves obtained by plotting the results of the different bservers lesser maxima and minima which showed peculiarities in the different eyes. These peculiarities appeared unchanged in every set of observations taken by the same person.

I wish to express my obligations to Professor Gibbs for his advice and for the loan of apparatus, to Professor J. P. Cooke, who kindly placed at my service the magnificent spectroscope made for him by the Clarks, and, among others, to Professor W. E. Byerly, Professor C. R. Lanman and Messrs. F. B. Knapp, E. B. Lefavour and E. S. Sheldon for their kindness in helping me with their observations.

Cambridge, Aug., 1883.

ART. XXXIII.—*Injury sustained by the Eye of a Trilobite at the time of the Moulting of the Shell;* by CHARLES D. WALCOTT.

MR. WILLIAM P. RUST, of Trenton Falls, N. Y., called my attention some time since to the eyes of a small but very perfect specimen of *Illænus crassicauda*, from the Trenton limestone, that he has in his beautiful collection of Trenton fossils.

The left eye is perfect; the visual surface is clearly defined, and in the sunlight almost translucent between the darker base and the curve of the facial suture above. The right eye at first sight appears to have been broken in working away the matrix, but a close examination shows, as Mr. Rust expressed it, that the eye had been put out while the animal was living. This is shown by the peculiar growth of the shell about the aperture formerly occupied by the visual surface of the eye. The margins are turned in, rounded and contracted, and the size of the palpebral lobe materially lessened. An injury to the visual surface would scarcely produce this effect if the shell was hard. If slightly injured before the moulting of the shell, the separation would be imperfect and the visual surface carried away with the old shell would leave a cavity around which the new shell would form as in the eye before us. If injured before the new shell had hardened, that effect might be produced, but the probabilities are that the loss of the visual surface occurred at the time of the moulting of the old shell.

Among the thousands of trilobites that have passed through my hands in which the eyes were preserved, I have never noticed any distortion or injury that occurred during the life of the animal. In a few instances, the shell of the pygidium of *Asaphus platycephalus* has shown evidence of local fracture that appears to have occurred during the life of the animal, but these were very unsatisfactory. To Mr. Rust's skill in working out the specimen described and also in detecting the character of the injured eye, we are indebted for some positive information of an injury sustained during the moulting of the shell of a trilobite.

ART. XXXIV.—*Dip of the Rocks in Central New York;* by Professor S. G. WILLIAMS.

FROM a careful study of the outcrops of the Tully limestone over an extent of more than eighty miles east and west, in the counties of Yates, Seneca, Tompkins, Cayuga, Cortland, Onondaga and Madison, data have been obtained for an approximate determination of the dip of the strata in that part of the state. This limestone, from its numerous bold outcrops in the nearly meridional valleys of the lakes and streams of the counties named above, forms a very convenient stratum for this purpose; whilst the elevation of the lakes of central New York above mean tide, and, beyond the region of the lakes, the best attainable railroad levels, have given the means of obtaining approximately the height of this great sheet of rocks above the sea level, at many important points, by observations with the aneroid barometer, often several times repeated and verified by readings at many stations.

The height of the north outcrop of the Tully between Seneca and Cayuga Lakes has been determined, by levelling, to be 739 feet above the sea, or 292 feet above Seneca Lake. From this point S.W. to the sharp synclinal bend figured in Hall's Report on the 4th District, at page 212, is four miles, in which the strata descend 252 feet or 63 feet per mile; or if we disregard this sharp local bend and two other smaller ones farther south, and extend the observation to the point of final disappearance in the town of Lodi, a distance of 7·8 miles 25° west of south, the dip is 37½ feet per mile. From this same point, which is 362 feet above Cayuga Lake, to Little Point on that lake, where the Tully is carried below the lake level by a synclinal bend and remains submerged for four and a fourth miles, is a distance of about ten miles S. 42° E.; the dip is therefore 36 feet per mile. Little Point is 4·73 miles S. 10° W. from King's Ferry on the east side of Cayuga Lake, where the Tully is 225 feet above the lake, giving a dip in this case of 47½ feet per mile.

From King's Ferry to Lake Ridge on the east side of Cayuga Lake, a distance of a little more than 4·4 miles S. 13° E., the strata descend 190 feet, a dip of 42½ feet per mile, and observations extended northward from King's Ferry show that this rate of dip holds good for more than six miles farther. From these data and others of the same purport, I judge that the dip of the strata in the Cayuga Lake region, north of the bend immediately to be mentioned, is at least 45 feet to the mile and in a direction probably about ten degrees west of south.

On the west side of Cayuga Lake south of Little Point, after the Tully has been submerged for more than four miles, it rises again from the lake and makes an arch of 4·4 miles span and 160 feet in extreme height, descending to the south at the rate of 80 feet to the mile and northward 67 feet per mile. On the opposite side of the lake, the limestone immediately north of the arch is nowhere fully submerged, but after continuing nearly horizontal for about three miles, rises into an arch of over six miles span, the highest observed point of which is 285 feet above the lake. From this point the dip to the south, which is much the most rapid, is 110 feet per mile. In his Report on the Third District of New York, p. 167, Vanuxem attempts to show that this arch is not a real upward bend of the strata but only an apparent one caused by a bend of Cayuga Lake to the southeastward. It is a sufficient answer to this theory to state that on the west side of the lake, the outcrops of the Tully, when laid down on an accurate map, for more than nine miles, including the whole disturbed region, do not deviate five degrees from a straight line. A line drawn through the lowest points of the synclinal on Cayuga Lake passes through the sharp synclinal bend on Seneca Lake before alluded to, and strikes the outlet of Crooked Lake near a point where the Tully has apparently suffered a fault of about a hundred feet. This line has a direction of about W. 27° N.

Disturbances similar to those on Cayuga Lake, but less in extent, occur on both sides of Seneca Lake, involving, however, in the vicinity of Lodi, two subordinate bends of a few feet each, which appear on both sides of the lake.

The northernmost observed outcrop of the Tully in the Owasco Lake valley is a mile west of Scipio station on Owasco Lake. This outcrop is 1110 feet above the sea, from which, to the lowest point of the synclinal on Cayuga Lake, there is a descent of 730 feet in about seventeen miles, or 43 feet per mile southwesterly. From the same point southeastward to the most southern exposure in the Owasco valley, there is a descent of 310 feet in nine miles, or 34 feet per mile. Some not very reliable data, extending over a space of nine miles between Owasco and Skaneateles Lakes, show an apparent dip of at least 40 feet per mile S.S.W, but there is good reason for believing that this is less than the reality. On Skaneateles Lake the dip of the Tully from its north outcrop near Borodino, Onondaga county, to Glen Haven, at the heed of the lake, is 210 feet in five and a half miles, or 38 feet per mile a little east of south. Hence there is no good reason for thinking that in the valleys of Owasco and Skaneateles Lakes the dip of the rocks varies materially from that in the Cayuga Lake region.

East of Skaneateles Lake but three useful points have yet been obtained and these only approximately. One of these, Pickett's Hill, in Fabius, Onondaga county, shows the Tully about 1680 feet above the sea. A second station, over a mile east of DeRuyter village, Madison county, is distant from the first six and a half miles in a southeasterly direction and is about 260 feet lower, a dip of 40 feet per mile. The third station, near the village of Cuyler, in Cortland county, is also six and a half miles from the first, a little west of south, and is about 365 feet lower, being a dip of 56 feet per mile. Hence it would seem that the dip of the strata increases somewhat toward the east, and is there also directed a little west of south.

The distances given in this article for the Cayuga Lake region were obtained by scale from an accurate map of Cayuga Lake made by the Engineer Department of Cornell University, and are doubtless correct. The remaining distances were taken by scale from Asher and Adams' Atlas of New York, verified by comparison with a large map of New York and with the best attainable county maps, and can be considered only as reasonable approximations; still the substantial agreement of results from data derived from several different sources makes me confident that they may be relied on as reasonable approximations to the dip of the strata over the region in question.

Cornell University, Ithaca, N. Y., Aug. 3.

ART. XXXV.—*Physiological Optics*; by THOMAS WILLIAM BACKHOUSE.

THERE are some points in Mr. W. LeConte-Stevens's papers, published in this Journal, to which I take exception, though other points I must confirm.

The papers go to disprove Brewster's theory that the mode of judgment of distance in binocular vision is by the optic angle, interpreted by means of triangulation. Some of Stevens's objections to this theory seem to me to be untenable. I gather that he regards LeConte's theory as insufficient, though he partly accepts it. I shall proceed to criticize some of Stevens's remarks.

Vol. xxiii, p. 291.—He discards the theory of visual lines and of corresponding retinal points, and substitutes that of associated muscular action. But if this be the true one, must it not *first* be derived from observation of the intersection of visual lines?

I understand the theory of corresponding points to be that the brain causes each point on one retina to appear to coincide with a point on the other, and that these coincident points are invariably the same. Stevens doubts this, though I do not well understand how far his theory of "partially correspondent retinal areas"* differs from it. But even in this modified theory he considers "this correspondence must be considered merely the effect of association resulting from oft-repeated experience." But surely experience would be far more easily learned if based on some anatomical or mathematical fact than if there were no such basis to go upon; and therefore it seems to me that there must be such a basis, and that it is consequently very improbable that the theory of corresponding points can be incorrect. Nevertheless, the facts he gives in volume xxiv, page 241, etc., and page 331, etc., perhaps point in this direction showing, as they do, that the brain is often unconscious of double images, and also that stereoscopic relief is obtained when the time is too short to allow much play of the muscles upon different objects in view. Indeed, when spectral after-images are binocularly viewed there can be no play of the muscles. Is it possible, then, that we judge of distance by the brain uniting similar points of the two images, *even when they are not on corresponding points of the retinas*, and judging by the distance from the actual corresponding points? LeConte's theory† seems more probable, viz: that we judge partly by observing, *perhaps often unconsciously*, that objects are double, and more accurately by moving the point of sight backward and forward by the muscles, converging the eyes successively on the different distances in sight. I think that observations on persons who habitually squint would throw much light on these questions; but they are not easily made.

Vol. xxii, p. 444.—From the experiment on viewing parallel lines by divergent vision it looks as though the brain judged more of the *relative* distance of different objects than of *absolute* distance. Stevens suggests this in volume xxiii, page 356, lines 1 to 4. Likewise, geometrical forms viewed in a stereoscope do not give any idea of absolute distance but only of relative distance. I take this to be owing to the conflict of focal and axial adjustment.

VolXXIII, Page 294, etc.—This paper shows that we can judge of distance pretty well when axial and focal adjustments agree, but not otherwise. This is not surprising when it is considered‡ that there must always be three elements that influence our judgment of distance, viz:

1. The optic angle, interpreted through the rectus muscles of the eye-balls.
2. The focal adjustment, interpreted through the ciliary muscles.

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* Vol. xxiii, p. 359.

† Vol. ii, p. 3.

‡ See p. 292 and vol. xxii, p. 360, though in the list in the latter place focal adjustment is somehow omitted.

The visual angle, or apparent size of the object.
the experiments in the paper in question these three ele-
s conflict with one another. It is therefore no argument
st Brewster's theory to find that the judgment is at fault.
f the eyes, by means of triangulation of the object, say
vely that it is at a certain distance (as in the experiments
ge 294), while the ciliary muscles say equally positively
it is at another distance, how is the brain to decide?
s it gives paramount weight to one mode of judgment it
naturally conclude that the true distance is somewhere
en the two. In the experiment on pages 295 and 296 on
attern combined by axial divergence,* there is, in addi-
the judgment by the visual angle to contradict the other
s. This is really judgment by parallax of motion, so that
wo modes of judging parallax clash—that by motion and
by triangulation (according to Brewster's theory.) It is
rprising that the former outweighs the latter. Brewster's
y is, therefore, not contradicted by the experiments. His
y is that the parallax of the object observed by the two
tells us its distance. In ordinary circumstances this must
with the much larger parallax obtained by moving from
lace to another; but in this experiment the two are con-
tory, the brain naturally prefers to judge by the larger
ax.

ge 297.—The apparent curvature of the "phantom wall"
probably be explained in the same way. I have tried to
e experiment as far as possible, but as my eyes have not
asticity of Stevens's (and probably LeConte's and Brews-
I have not had much success; it is therefore only a sup-
on that these observers focussed their eyes upon the real
The effect of this would be that while the ciliary mus-
ould tell them the image was at the true distance of the
the rectus muscles (or the triangulation), would say that it
great deal nearer, and therefore they would judge that it
eally at an intermediate position. Now, while (as stated
ge 297) "it is an obvious geometric necessity that the
of the phantom surface must be a plane parallel to the"
this is only true as long as it is judged to be at the dis-
calculated by triangulation. If it is judged to be more
it, and the angle enclosed by the optic axes therefore
er than it really is, it will be a geometric necessity that
rface will appear curved, convex to the eye.

e nearest approach I can make to this experiment is as
s: I hold a small object, say a thin stick, between my
nd a wall paper, on which is a regular pattern, in such a

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en there is optic divergence, as it represents an impossibility, of course it
nflict with the focal adjustment.

way that the stick comes between one eye and one part of the pattern, and between the other eye and a corresponding part of the pattern. By looking at the stick a short while, especially if it is moved about in or near the plane parallel with the wall, the pattern is soon seen as a phantom at the exact distance from the eyes that it should be by triangulation, i. e., at the same distance as the stick is when placed as described above. The eyes are focussed on the stick, and therefore the phantom is very much out of focus; but, inasmuch as there is no dissociation between the focal and axial adjustments, the brain does not judge that the phantom is out of the focus but that the pattern is really of an indefinite character, with all its outlines softened and irregularities toned down. I assume that the difference between this experiment and that of Brewster, LeConte and Stevens lies in the focal adjustment. The result is that the wall generally appears to me perfectly flat.

In three of the observations, when the optic angle was between 34°·1 and 45°, I suspected a slight convexity of the phantom; indeed, in one case I thought it decided; but in another, when the optic angle was 31°, I at times suspected that it was slightly concave. In all other cases (up to an optic angle of 42°·9), it was perfectly flat. In some cases, usually with a large optic angle, there was a slight appearance of curvature in the straight lines on the wall, convex to the point of sight: but, on the other hand, in two cases I suspected they were concave to the point of sight. This appearance of curvature in the straight lines was sometimes connected with an appearance of curvature of surface, and sometimes with a flat surface. I cannot account for the illusion, which did not follow any perceptible law; it may perhaps have been caused by the patterns used.

The largest optic angle I could observe with was 45° (distance of phantom 3·04 inches). Stevens does not say how large an angle is necessary to produce the effect of a convex surface, so it may be that my want of any decided success is owing to not having compassed a large enough angle.

In these experiments the nearest point at which I could see an object in perfect focus was 4·5 inches (optic angle 31°·0), and if my theory is correct it may be that the suspicion of convexity with large optic angles is owing to the stick not being in focus; i. e., to its being impossible for me to obtain a focal adjustment corresponding to the axial adjustment, though it is very probable that the strain on the ciliary muscles in endeavoring to bring it into focus is sufficient to prevent the judgment of distance by means of the optic angle from being materially warped by that through the ciliary muscles.

If the eyes range about over the phantom wall and the stick s also moved till it is apparently placed exactly in the plane of he phantom, I have found that it is then exactly parallel with he real wall. At least, in eight independent observations and neasurements of its position, the maximum deviation from parallelism was only 1° 10′, and the average deviation 30′, which s not more than would be expected from errors of observation.

After getting the eyes well accustomed to the phantom it is possible to observe it for some little time after taking the stick away (especially if the optic angle is not large), before the eyes relapse into the position for viewing the wall naturally; during this time the focus is apt to lengthen, but the phantom continues to appear flat.

Vol. xxiii, pp. 298–301.—These experiments are of quite a different character from that of the phantom wall, inasmuch as the optic axes are nearly parallel, thus essentially changing the conditions. For the brain is perfectly conscious of the difference, and judges accordingly. It is most unlikely that the curvature of the retina (p. 298),[*] has anything to do with the phenomenon, which is easily explained on page 299, without reference to it. But the explanation of the effect on the points F and G appears to be incorrect, the fact being that combination is effected not between corresponding points on the two circles but, since a circle always appears as an ellipse in perspective whatever way it is viewed, *different* points in the two re combined, viz: points having the *same* retinal latitude.

I have not access to Rogers's paper on the combination of oints differing in latitude, but I apprehend this can be effeced only by moving the optic axis of one eye higher up than hat of the other, so that the images of the two points still fall n corresponding points of the retina. As I have just shown, he experiment here described does not bear on the subject, nd therefore cannot in the least invalidate the theory of coresponding points.

Vol. xxiii, p. 357.—I have seen stereographs of the moon showing it in relief, but these are always obtained by combining two photographs taken at *different times* when the moon's face by its rotation has been brought into a different position. I have never seen a stereograph of the sun, but if Stevens's statement is correct I have no doubt that the one he alludes to was produced in the same way. I do not dispute Stevens's remark that a very minute difference between two pictures is sufficient to produce the effect of relief; but these statements about stereographs of the sun and moon, if not explained, are calculated to give an exaggerated idea of this.

Sunderland, July 11, 1883.

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[*] And vol. xxiv, p. 243.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Possible Variability of the Law of Definite Proportions.*—In the July number of this Journal (page 63 of this volume), Professor Barker has given a very full summary of the views of Boutlerow on this subject, which have been recently presented to the Chemical Society of Paris by Wurtz, and emphatically endorsed in the discussion which followed by Schutzenberger. The opinion was then and there clearly expressed that the "chemical value" of a constant weight (or rather mass) of a substance might vary, and that the so-called atomic weight of an element might be simply the carrier of a certain amount of chemical energy which is variable within narrow limits; and further the question was asked whether "Prout's hypothesis may not be a true law which like that of Mariotte admits of a limited variation." Moreover numerous facts were cited, chiefly results of quantitative analysis, which seemed to confirm the views thus expressed. Such opinions are certainly very revolutionary, and, if they prevail, must entirely change the fundamental conceptions of chemical philosophy. Chemical combination can no longer be regarded as the juxtaposition of the definite invariable masses we call atoms, but must be considered as the "reciprocal saturation" or "interpenetration" of masses which may vary with the relative strength of their chemical energy acting at the time; and this change of the fundamental conception is inconsistent with the atomic theory and with the superstructure which modern chemistry has built upon it.

In the connection above referred to it is stated that Boutlerow had held the views then advanced for the past three years. Such views are, however, by no means new. They were not only expressed, but fully worked out by the writer more than twenty-five years ago. They were presented to the American Academy of Arts and Sciences in 1855 and fully developed in a paper published in the Memoirs of this Academy (New Series, vol. v, p. 337) "On Two New Crystalline Compounds of Zinc and Antimony and on the Cause of the Variation of Composition observed in their Crystals." The same subject was also discussed in a paper published in this Journal, "On an Apparent Perturbation of the Law of Definite Proportions observed in the Compounds of Zinc and Antimony" (Second Series, vol. xx, 1855). In these papers the opinions now under discussion were brought forward, not simply as speculations, but as a legitimate theory, which was advanced to explain the facts observed in a long and laborious investigation of the order of the variation in the composition of certain well-defined crystals, obtained by the author from alloys of zinc and antimony of different percentage composition by the "method of fusion." Alloys were crystallized differing by from one-half to five per cent, according to circumstances, from the alloy containing 95 per cent of zinc to that containing 95 per cent of antimony, and analyses were made of the crystals thus obtained. Two well-marked types of crystals, and only two, were obtained corresponding to the composition Sb_2Zn_2 and Sb_2Zn_3; but it was found that while crystals of the first type might be made either with an excess of zinc or an excess of antimony, the crystals of the second type would only take an excess of zinc; and the whole order of the phenomena indicated—first, the action of a force tending to unite the metals in one of two definite proportions; secondly, the effect of the excess of one or the other metal in the alloy tending to disturb this result; thirdly, the interference of the two compounds with each other in alloys of intermediate composition. The results were plotted and a curve drawn showing the order of the variation, and this curve indicates plainly—first, the strong tendency of the chemical force, and secondly the disturbing effect of the excess of either metal in the alloy. Moreover, the manner in which these two influences modified each the other's action seemed to preclude the supposition that the results could be the effect of a simple mechanical enclosure by the growing crystals. For all these details we must refer to the papers cited. We will only quote a few sentences which indicate the drift of the writer's opinions at the time.

"In the absence of any known principle of chemical science by which the remarkable variations of composition that have been demonstrated in this memoir can be explained, the conclusion is almost forced upon us that zinc and antimony are capable of uniting and producing definite crystalline forms in other proportions than those of their chemical equivalents; in other words, that the law of definite proportions is not so absolute as has been hitherto supposed."

"Accepting the view of the subject that has been offered, it will be obvious that the very large extent of the variation in the compounds of zinc and antimony is due to the very weak affinity between these elements. Were the chemical force stronger in proportion to the disturbing force, the variation would be lessened; were it weaker, the variation would be increased."

"To what extent this perturbation of the law of definite proportions prevails among chemical compounds, it must remain for future investigation to determine. There are, however, a number of facts which tend to prove that it is very general whenever chemical affinity is weak." Examples are cited in the memoir.

"If variations in composition of such magnitude are possible when the force of chemical affinity is weak, it is highly probable that some variation may occur when the force is strong; and whatever view may be taken of the cause of the variation, it will now become a matter of importance to ascertain whether many discrepancies of analysis hitherto referred to imperfections in the process may not be owing to the same cause which influences the composition of the crystals of the antimonides of zinc.

"The definite proportions I regard as a maximum toward which the chemical force strives, a maximum from which the deviations in most cases are small, although in others they may be very large; and I maintain that this view of the subject, which the memoir has aimed to establish, is supported by the analogies of nature.

"I have shown in a former memoir that these numbers" (the equivalent numbers) "may be connected by a very simple numerical law, but here, as in other cases, we find merely a tendency towards the law, not an absolute agreement with it, the differences between the theoretical and the experimental equivalents being in many cases too great to be covered by errors of observation. The present memoir may throw light upon these discrepancies; for, to say the least, it is possible that the differences may originate in variations of the equivalent itself, and that the theoretical equivalent may be the maximum towards which the chemical force tends."

In a subsequent paper entitled "Crystalline Form not necessarily an Indication of Definite Chemical Composition, or on the Possible Variation of Constitution in a Mineral Species independent of the Phenomena of Isomorphism," published in the Philosophical Magazine (June, 1860), during a visit to London, the writer shows that whatever explanation may be given of the variations of composition, which he had previously described, such variations must necessarily modify the composition of many crystalline mineral products. Examples are cited in which this is obviously the case, and it was proposed to designate by the word "Allomerism" "a variation in the proportions of the constituents of a crystallized compound without any change in the crystalline form."

Now when after twenty-five years the same question is revived, although without any reference to previous work on the subject, and the same opinions are expressed under conditions more likely to give them currency, it becomes interesting to enquire whether the progress of chemistry during the interval has tended to confirm or invalidate these views. So far as the writer is aware no investigation, except his own already referred to, has been directed to the exact point in question, and in none of the analytical results which have been more recently cited as possible examples of variation in the combining proportions has it been conclusively shown that the discrepancies may not result from causes such as will be considered below.

During the last twenty-five years, however, a more extended knowledge has been acquired of that class of indefinite chemical compounds which like the alloys seem to occupy an intermediate position between definite composition and solution. Such compounds for example as many of the hydrates and silicates in which it is impossible to mark the limits between chemical and mechanical action. As in the case of the crystalline compounds of zinc and antimony, so in all such cases, some mechanical ac-

ion,—such as is manifested in what we call solvent power,—seems
o modify the chemical force: but in no case except the first
as any attempt been made to investigate the interaction of
he two. The atomic theory explains such cases of indefinite com-
ination by distinguishing between atomic and molecular union,
nd it regards the complex products as resulting from an aggre-
ation of dissimilar molecules, in each of which, however, the law
f definite proportion rigorously holds. It is necessary, how-
ver, in many cases to assume the existence of very complex
nolecules, and the doctrine of interpenetration certainly gives a
impler conception of this class of phenomena.

On the other hand the extension of our knowledge has either
rought to light new facts or given prominence to old ones,
hich, by explaining anomalies, have tended to confirm the atomic
eory. It has been found that crystals have a very remarkable
ower of enclosing foreign materials in their structure, so that
times the crystallizing material forms but a small portion of
e resulting mass. If a crystal may enclose material wholly
reign to its composition, why may it not enclose an excess of
ther of its ingredients? There can be no doubt that in such
ses the amount of material thus enclosed depends to a greater
less extent on the chemical force, and there is a continuity in
e composition of the products which makes it difficult to dis-
nguish between the material controlled by the chemical force
d that which is not, and gives at least the appearance that the
nount so controlled may be variable.

On the assumption, however, of the existence of molecules and
oms it is a very probable hypothesis, that when we bring to-
ether in solution or in fusion substances capable of yielding defi-
ite compounds, the number of molecules of such compounds
at would form *at any one time* must depend—certainly among
ther conditions—on the strength of the chemical force as well
s on the proportion of the several materials present. When,
ow, crystals form in such a menstruum we should expect that
hile the molecules of the definite compound would aggregate
ogether, the crystalline structure thus formed would enclose, to
greater or less extent, molecules of one or the other of the constit-
ents of this compound if present in excess, and the amount of such
nclosure must depend on the relative proportion of the molecules
n the menstruum; and this proportion, as we have seen, must have
een already determined by the strength of the chemical force.
hus might arise such a regulated variation of composition as the
riter's investigation indicated. The extent of the formation of
nolecules of definite composition in the menstruum would obvi-
usly be regulated by conditions, whose action would determine
o break of continuity; but the segregation of these molecules in
he formation of crystals would cause at once a sudden change.
rystallization is always a break of continuity, and although the
rystalline structure may, as we have seen, enclose foreign ma-
erial to a very large extent, yet, as the writer's investigation

proved, there is always, even in extreme cases, a tendency to exclude such material, a tendency which is the more effectual in proportion as the crystalline molecules preponderate in the menstruum. Hence it is that by repeated crystallization perfect purity may, in most cases at least, be secured. The hydrates of sulphuric acid furnish a good illustration of the theory we are discussing. Water and oil of vitriol may be mixed in any proportion and there is a perfect continuity between the various stages of dilution. When however on exposure to cold the definite hydrate $H_2SO_4.H_2O$ crystallizes out this continuity is abruptly broken, and it is reasonable to suppose that the molecules of this compound preëxisted in the liquid and only aggregated in the process of crystallization. These crystals may be obtained between quite wide limits of dilution, and must enclose more or less of the menstruum, and, therefore, vary in composition to a limited extent; but the definite hydrate can be obtained pure by repeated crystallization.

Another class of facts bearing on the same subject has been forced upon the writer's attention during the last few years by the investigations he has undertaken on the revision of the atomic weights. In a memoir on the "Numerical Relations between the Atomic Weights," which he communicated to the American Academy of Arts and Sciences, February 28th, 1854, he remarked —as the result of a careful discussion of the determinations which had been made up to that time—that the constant errors "seem to be the great errors in all these determinations, those accidental errors which were made in the repetitions of the same process by equally careful experimenters being comparatively insignificant." The writer's subsequent experience has entirely confirmed this opinion.

Of these constant errors by far the most important are those due to impurity of material, whether in the substance analyzed or in the products of the analysis. No problem of chemistry is more difficult than to prove that the materials and products involved in atomic weight determinations are perfectly pure, and represent a condition in which the elements are united in the definite proportions sought. It is of course very easy to discover such impurities as can be detected by well known analytical processes. But how can you be sure that a given oxide, sulphide, chloride, bromide or iodide does not contain a similar compound of a lower or higher order? How can you be sure that either of these compounds does not contain a small amount of oxide? It is such impurities as these that are the chief source of uncertainty, and they cause variations of composition precisely similar to those discussed above. By referring to the examples cited on page 65 of this volume it will be found that they are for the most part, if not all, open to a suspicion of errors of this kind, and until these suspicions are removed the evidence which the alleged discrepancies furnish is not trustworthy. As a rule, when a substance is formed by a simple reaction from perfectly pure materials, the purity of the product is taken for granted, and yet the writer has shown that sulphide of antimony, when precipitated by hydric sulphide from tartaric acid solutions acidified by hydrochloric acid, invariably contains more or less oxichloride of antimony, which leaves its equivalent of oxide of antimony when the sulphide is dried at 200° C. So also he found it impracticable to prepare chloride of antimony free from oxide.

It may then well be asked what proof can we have of purity in such cases as have been discussed beyond the very definiteness of composition which is in question? Before attempting to answer this question the writer would call attention to the fact that constancy of analytical results affords no certain criterion of purity. In the writer's own experience constancy of results has frequently meant nothing but constancy of impurity, and the illusion caused by coincidences so-called has been forced again and again on the writer's attention. Hence no valuable conclusion can be reached by attempting to combine, according to the method of least squares, results of atomic weight determinations made by different chemical processes. The writer undertook such a discussion, as stated in his early memoir on the relations of the atomic weights, above referred to, but abandoned it simply because accidental errors are the only ones which such mathematical methods can eliminate and these were found to be quite insignificant factors in the problem. No useful conclusion can be deduced by comparing different classes of results, each affected by unknown constant errors; for it is impossible to assign to them correct values and in comparison with results from which constant errors have been eliminated they are all worthless. For example, in the writer's published work on the atomic weight of antimony, although a closely accurate result was obtained by the synthesis of the sulphide of antimony, this result is of no value in comparison with that obtained subsequently from the analysis of bromide of antimony, and to assign it any value in a discussion of probable errors would lead to error.

If now the question is still pressed: what is the criterion of a pure and definite compound? the only explicit answer that can be given is that a substance obtained by repeated crystallizations offers the most uniformly satisfactory evidence of purity that we possess. It is well known, however, that repeated crystallizations will not always ensure purity, and that very frequently the crystals, though pure when formed, cannot be collected and preserved without change. Moreover, it is seldom the case that such repeated crystallizations are possible. In most instances we must rely on other criteria of purity, such as constancy of physical properties like the melting or boiling points, and often the question can only be answered by a special, frequently a difficult, investigation. Indeed herein lie the great obstacles which we meet in all attempts to fix with accuracy the values of the atomic weights, and good judgment is the only criterion on which we can always depend.

It is surprising how few substances fulfill the conditions required in an accurate atomic weight determination, rarely more than one compound of each elementary substance; and for some of the most common elements, so far as we yet know, not even one. After the process has once been worked out, the actual analytical work is generally readily accomplished and the accidental errors involved are comparatively small; and when the constant errors have all been eliminated the definiteness of the results is surprising; and cannot but produce in the analyst a confidence in the definiteness of the law which determines such effects. With the writer, while his earlier work on crystalline alloys left an impression of the possible indefiniteness (under certain conditions) of chemical combination, his later work on atomic weights has shown him the exceedingly definite aspects of the same phenomena. Moreover, he has not found any evidence that the definiteness of chemical combination diminishes as the chemical force is weakened, a result we should expect if a variation were possible and as was predicted in the memoirs that have been so often cited. On the contrary, the writer has found that when purity is secured the proportions are as definite in weak compounds as in strong, in oxide of silver as in water; only with weak compounds it is usually more difficult to ensure purity.

While therefore the writer feels that the weight of evidence is at present in favor of the atomic theory, and of that absolute definiteness of combining proportions which this theory involves, still he is very glad that the whole question has been reöpened in such an authoritative way as is likely to lead to its solution. His own investigations, now in progress, will have a direct bearing upon it, and until the requisite facts have been established there is no sufficient ground for a positive opinion. But, although it must be admitted that the atomic theory is the only basis on which a consistent philosophy of chemistry can at present be built, the writer must confess that he is rather drawn to that view of nature which refers all differences between substances to dynamical causes, and which regards the atomic theory as only a temporary expedient for representing the facts of chemistry to the mind.

J. P. C.

2. *On the Spectrum of Beryllium.*—The position of beryllium among the elements still remains doubtful. Nilson and Petterson regard it as a triad with an atomic weight of 13·8 and a specific heat of 0·4079 to 0·4083, its oxide having the formula Be_2O_3. Emerson Reynolds, on the other hand, believes it to be a dyad, with an atomic weight of 9·2 and a specific heat of 0·642 (calculated), and assigns to the oxide the formula BeO. The specific heat, as determined experimentally by the former chemists, is somewhat uncertain since the metal employed contained from 5·59 to 13 per cent of impurities and was not homogeneous, being partly crystallized and partly in fused globules. To throw additional light upon this question HARTLEY has examined the spectrum of beryllium. If a triad, its spectrum should show consider-

able analogy with those of aluminum and indium; though if a dyad, the spectrum, in accordance with the periodic law, would not necessarily coincide with those of magnesium, zinc and cadmium, the other members of the homologous series. The spark spectrum of beryllium was obtained from a saturated solution of the chloride prepared from pure oxide. No lines belonging to other elements were seen in the spectrum except two very faint ones of calcium from the hydrochloric acid used. The spectra were produced with a prism and also with a Rutherfurd grating of 17,460 lines to the inch, and were photographed with a camera, the lens of which, as also that of the collimator tube, was 36 inches focus. Seven lines were observed, two of which were in the visible spectrum; but of these only one was photographed and that was too faint to be measured. Of the five lines photographed and measured, the third, of wave-length 3320·5, the ifth, 2649·4, the sixth, 2943·2, and the seventh, 2477·7, were trong and sharp, while the fourth, wave-length 3130·2, was very trong, still more extended and the most persistent of all. As a esult of his comparisons the author says: "The spectrum of eryllium exhibits no marked analogy with the calcium, the magesium or the aluminum spectra, all of which are members of well-defined homologous series. There is nothing similar to the oron, the silicon or carbon spectra, nor to those of scandium, ttrium or cerium. The spectrum of lithium is the one most llied to that of beryllium in the number, relative position and ntensity of the lines. The character of the lines of greatest inensity in the beryllium spectrum is more like that of the pairs of ines in the spectrum of calcium, with wave-lengths 3933–3967·5 and 3705·5–3736·5. In fact the similarity to the line 3705·5 is so great that the two when juxtaposed on one plate may be mistaken if their positions be not taken into account. Lines of similar elenents have very distinctive features. Individual lines of the calcium, strontium and barium group are quite different from those of the magnesium, zinc, cadmium series, and they could not be mistaken. The lines of cerium and didymium again are quite dissimilar in character; instead of being broad they are exceedingly fine. I am therefore led to the conclusion that beryllium is the first member of a dyad series of elements of which in all probability calcium, strontium and barium are homologues."—*J. Chem. Soc.*, xliii, 316, June, 1883. G. F. B.

3. *On a Relation between the Density and the Chemism of the Elements in different Allotropic states.*—MÜLLER-ERZBACH has called attention to the apparently necessary connection between the activity of a chemical reaction and the amount of volume-contraction which takes place during its progress. Since the reaction is the more energetic in proportion as the condensation is greater, it follows that in the case of those elements which exist in different allotropic states, the chemism should be a maximum in those forms whose density is furthest removed from that of the final product, which of course is the same for all the vari-

eties. In the case of sulphur, the rhombic form has a density of 2·07 and the amorphous of 1·92; now the latter is the more readily attacked by nitric acid and permanganate, combines with nascent hydrogen more readily, and is more active in forming trithionic acid, as Berthelot has shown. Amorphous red selenium, according to Schaffgotsch, has a density of 4·26, the vitreous variety a density about 4·28, and the black granular or metallic selenium one of 4·8. But experiment does not show any marked difference between these varieties as regards their chemical activity. In the case of phosphorus, however, the result is well-marked. Common phosphorus has a density of 1·826–1·840, according to Schrötter, while that of the red variety is 2·106 (2·19 as given by Hittorf), and of the crystalline metallic 2·34 (Hittorf). Now ordinary phosphorus combines with oxygen with ignition at 60° and with sulphur at a little below 100°; while red phosphorus requires temperatures of 260° and 230° respectively. The same differences in activity are observed toward other elements, common phosphorus even reducing copper, silver and lead from their solutions. Carbon as diamond has a density of 3·5, while the density of graphite varies from 2·1 to 2·6, and that of amorphous carbon from 1·4 to 2. When treated with a mixture of potassium chlorate and strong nitric acid the diamond is absolutely unattacked, the graphite yields graphitic acid slightly soluble in water, while the amorphous carbons give humus-like bodies and dissolve completely. The relative combustibility of these varieties is proof in the same direction. According to Violette, charcoal prepared at 350° has a density of 1·50 and inflames at 360° to 370°; that made at 432° a density of 1·709, and ignites at 400°; that heated to 1250° a density of 1·86 and an igniting point of 600° to 800°; and that prepared at the fusing point of platinum a density of 2·00, and inflames at 1200°. Similar phenomena are observed with the various forms of silicon, boron, arsenic and tin. In no case does the chemism increase with the density. The author, therefore, concludes that the activity of the chemical process stands in direct relation to the increase in density which takes place during chemical union.—*Liebig's Ann.*, ccxviii, 113, April, 1883. G. F. B.

4. *On the behavior of Nascent Hydrogen in presence of Oxygen gas.*—TRAUBE has given his views on the production of hydrogen peroxide which differ radically from those of Hoppe-Seyler. When, for example, water acts on zinc at ordinary temperatures in presence of oxygen, zinc hydrate (not zinc oxide) is formed together with hydrogen peroxide, according to the equation

$$\mathrm{Zn}+\begin{matrix}\mathrm{OHH}\\\mathrm{OHH}\end{matrix}+\mathrm{O_2}=\mathrm{Zn(OH)_2}+\mathrm{H_2O_2}.$$

Whence Traube concludes that hydrogen peroxide is not an oxidation product of water (water not being directly oxidizable), but a compound of the unbroken oxygen molecule with two detached hydrogen atoms. In further proof of this position he

es the fact that in the voltaic decomposing cell the hydro-
roxide never appears at the positive electrode, where the
ctive oxidation takes place and where the water must be
d by the active oxygen were it capable of it, but at the
e electrode where the process is one of reduction and in
e of only passive oxygen. Hydrogen peroxide is there-
reduction product of the passive oxygen molecule. More-
lirect experiment shows that nascent hydrogen evolved
inc and either sulphuric acid or sodium hydrate does not,
ence of oxygen, cause the oxidation of either sulphindi-
cid or of ammonia; and that, as Graham had already
d, hydrogen-palladium gives up none of its hydrogen at
y temperatures, even in a vacuum. The oxidizing action
rogen-palladium is due to its specific action not to the evo-
of nascent hydrogen. Traube therefore denies the asser-
Hoppe-Seyler that nascent hydrogen has the property of
ng oxygen active. True, neutral solution of potassium
and starch is blued by palladium-hydrogen and oxygen;
s is due to the fact that hydrogen peroxide is first formed
en this, in presence of the palladium, sets the iodine free.
s in presence of platinum sponge, hydrogen peroxide
otassium iodide and starch and guaiacum tincture, and
s nitrite to nitrate, as observed by Schönbein. Traube finds
e peroxide, which alone is indifferent to carbon monoxide,
s it easily in presence of hydrogen peroxide. The oxida-
sulphindigotic acid is similarly accounted for. To show
author takes seven grams of palladium well charged with
en and agitates it for a quarter of an hour with air and
ater made intensely blue with $\frac{1}{1000}$ of potassium sulphin-
e. The blue color decreases but slightly and hydrogen
le is formed abundantly. If now the solution be poured
the metal and allowed to stand with exclusion of air, it
s gradually decolorized by the action of the H_2O_2, a clear
at the oxidation is not due to the direct action of the pal-
hydrogen. Traube therefore comes to the conclusion,
palladium-hydrogen does not evolve active hydrogen;
nascent hydrogen does not make oxygen active; (3) that
issues do not evolve hydrogen, and (4) that the peculiar
of palladium-hydrogen depends upon the formation of
en peroxide, which produces the oxidation, in part directly
part by the aid of metallic palladium.—*Ber. Berl. Chem.*
vi, 1201, May, 1883. G. F. B.

ew form of Electrical Accumulator.—JULIUS ELSTER and
EITEL show that Zamboni's dry piles can be used as accum-
The copper pole of the pile is connected with the posi-
l the tin pole with the negative poles of a Holtz machine.
e latter has been worked for a few minutes the dry pile is
o be charged. After repeated discharges the pile is found
ain a charge of considerable intensity. The authors rec-
l the following form of pile. The plates of the pile are

strung by means of a needle upon a silk thread and then stretched between the poles of a Holtz machine. A pile of 11,000 pairs of plates of one square centimeter surface, after ten minutes charging, gave shocks one millimeter long and made a Geissler tube luminous. The light of the tube was continuous at first but afterwards became intermittent. Dry piles were also made of one metal. Plates of lead foil were coated on both sides with tissue paper by means of potash water-glass to which a little oxide of lead was added. A pile of 7,000 of the lead plates one square centimeter in section could be charged so as to exhibit strong polarization. A certain amount of moisture must be communicated to the piles. The superoxide of lead deposited electrolytically acts more powerfully than when deposited in any other way. A pile of 1000 plates, coated on one side with chemically produced superoxide and on the other with protoxide of lead, gave proportionally much less tension. These piles are well suited to exhibit to a large audience the principle of Planté's or Faure's accumulator.—*Wiedemann's Annalen*, 1883, No. 7, pp. 489–491. J. T.

6. *Apparatus for the Demonstration of Foucault's Currents.*—Dr. A. VON WALTENHOFEN suspends between the poles of an electro-magnet a pendulum bob which is made in the form of an arc of a circular ring. The pendulum vibrates in a direction perpendicular to the axes of the poles of the electro-magnet. Before the electro-magnet is made the time of swing of the pendulum is noted and compared with the time of swing when the current is passing through the electro-magnet. In the latter case the diminished number of swings can be clearly seen by a large audience. This method is capable of quantitative measurement.—*Wiedemann's Annalen*, 1883, No. 8^b, pp. 928–932. J. T.

7. *Dependence of the Magnetising Function of Steel upon its Hardness.*—HUGO MEYER reviews his numerical results upon this question—the magnetizing function being the magnetic moment divided by the magnetizing force—and considers the physical effects produced by the presence of a hardened layer of steel surrounding a core which is less hardened. He makes various suppositions to reconcile his results with the hypothesis that a piece of steel is made up of small magnets which are turned in their beds when the bar is magnetized. His suppositions are so unlikely that doubt is cast upon the molecular theory of magnetism.—*Wiedemann's Annalen*, 1883, No. 8^b, pp. 849–856. J. T.

8. *Experiments bearing upon the Electro-magnetic Theory of Light.*—In a voluminous paper containing a large series of observations, QUINCKE endeavors to verify Maxwell's conclusion that the square root of the dielectric constant must be equal to the index of refraction for light of the same substance. The several methods adopted give results which are not in accordance with Maxwell's theory of light. Quincke explains the different results obtained by different observers upon the effect of electric force on the index of refraction of certain substances by the following:

(1.) Experiments show a fluctuation in the value of the index of refraction which is due to the electric force.

(2.) A comparatively long duration of the electric pressure causes a fall in the value of the index of refraction equivalent to the effect of a rise of temperature between 0·0001° to 0·1° C. This increases with the difference in potential between the electrodes, and can be attributed to internal friction caused by electric attractions and repulsions between the particles of the fluid.

(3.) The phenomena of the change of the index of refraction show that the electric pressure has no analogy with hydrostatic pressure.

(4.) These changes in the index indicate changes in hydrostatic pressure in the interior of the fluid which are caused by the electrical pressure, the fluid being set into vertical movements thereby.

(5.) The electrical effects appear to be transmitted through the fluid by impulses and not in a continuous manner.—*Wiedemann's Annalen*, 1883, No. 8^b, pp. 705–782. J. T.

9. *A method of determining the Ohm.*—In a valuable paper, entitled "On the Determination of the Intensities of powerful Magnetic Fields," by Mr. A. GRAY, which is too technical for a short abstract, the author proposes a method of determining the ohm which is nearly the converse of that of Weber. The method is to hang a coil, of which the constants are known, in a sufficiently intense and uniform magnetic field and find the decrement of the oscillatory motion produced by the induction. The author believes that the calculations and corrections would be, in certain respects, simpler than the method given by Weber.—*Phil. Mag.*, Aug., 1883, pp. 144–146. J. T.

10. *A new Seismometer.*—Professor EWING, of Tokio, Japan, proposes the following form of Seismograph: A common pendulum, having its center of gravity below its point of suspension, is stable. An inverted pendulum with a pivoted supporting rod is unstable. By placing an inverted pendulum below a stable one and connecting the bobs so that any horizontal displacement will be common to both, the system can be made as neutral in regard to stability as one may desire. The instrument has not, however, been tested in an actual earthquake.—*Nature*, July 26, 1883, p. 308. J. T.

11. *Infra red spectra.*—M. HENRY BECQUEREL describes a method of studying the spectra emitted by the vapors of sodium, magnesium, calcium, potassium, silver and thallium which opens a field of observation between the wave-lengths 760 and 1300, an interval greater than that between the extreme red of the visible spectrum and the last ultra violet rays.—*Comptes Rendus*, July 9, 1883. J. T.

II. GEOLOGY AND BOTANY.

1. *Paramorphic Origin of the Hornblende of the Northwestern States*; by R. D. IRVING.—The writer of the note communicated to the August number of this Journal, referring to my paper on the above subject in the July number, says that I seem

to imply therein "that no one except Dr. Wichmann has ever studied microscopically the rocks of the Marquette district, and no one except myself advocated the secondary origin of the hornblende in the 'greenstones' and the eruptive origin of the diabases." Just what greenstones and diabases are here referred to I do not understand, but in reply I have to deny that the paper was ever intended to contain any such implications; nor can it be construed, so far as I can see, to contain them.

The writer of the note also says that I have advocated, without acknowledgment, his previously published views as to the "trachytic and rhyolitic nature of the Keweenaw felsites." To this I have to reply that I have never held the view which he attributes to me. In the Keweenaw series occur, as I was the first to announce, so far as I am aware, original masses, not only of basic but also of acid eruptives, and of eruptives of intermediate acidity, the various kinds constituting a continuous series from the most basic to the most acid. In their general relations, and to a certain extent in their general characters, these rocks correspond to the Tertiary basalts, trachytes and rhyolites, but I have never held the view that any of these ancient rocks are lithologically identical with the Tertiary rhyolites and trachytes, though the words used in the second annual report of the Director of the U. S. Geological Survey might possibly be so construed as to mean this. My acquaintance with the modern eruptives is not yet great enough to warrant the holding of such an opinion against the accepted views of the most eminent lithologists of the day.

What the writer means to imply by his reference to the peridotite of Presq' Isle I do not understand—as I have never published anything, so far as I know, directly or indirectly referring to this rock.

Finally, in reply to the general summing charge of the writer of the note, I have merely to deny that I have ever published as original with myself any of his previously published views. And the truth of my denials will, I am sure, be quite apparent to candid minded persons acquainted with the literature of the subject, to whose judgment I am quite content to leave the matter without further argument.

2. GEORGE VASEY: *The Grasses of the United States; being a Synopsis of the Tribes and Genera, with Descriptions of the Genera and a List of the Species.* (Department of Agriculture; Special Report, No. 63. Washington: Government Printing Office. 1883.)—As Botanist of the Department of Agriculture, as well as by predilection, Dr. Vasey is particularly interested in the *Gramineæ*, and has studied them most assiduously. Availing himself of Mr. Bentham's recent elaboration of the order in the new Genera Plantarum, he has now given, in English, a succinct conspectus of our grasses, characters of the 114 genera known to be North American, and a catalogue of 589 species, indicating their geographical distribution. All this in a pamphlet of forty-seven pages, which is distributed by the Agricultural Department

at Washington, so that it may be in the hands of all our botanists, to whom it will be a seasonable and considerable help. A. G.

3. SERENO WATSON: *Contributions to American Botany. XI.* (Extract from Proceedings of the American Academy of Arts and Sciences, vol. xviii, issued August 15, 1883).—A year ago, in the seventeenth volume of the American Academy's Proceedings, Mr. Watson brought out the tenth part of his important Contributions, half of which was occupied with the determination of the Polypetalæ of Dr. Edward Palmer's collection in northern Mexico and the adjacent part of Texas. This account is completed in the paper now before us, of which it occupies about one hundred pages. The new *Compositæ* are enumerated, but their characters are given in a later-published paper by another hand. In other orders there are a goodly number of new species, some of particular interest.

Omphalodes aliena and *O. cardiophylla* Gray, published in Hemsl. Biol. Centr. Am., are certainly very remarkable, as representing that genus in the New World, as also for the more extended insertion and sometimes plane wings of the nutlets; so that they truly "seem to belong to *Paracaryum* rather than to *Omphalodes*, if these genera are to be retained as characterized by Bentham and Hooker." But, indeed, they were referred to *Omphalodes* on the idea that *Paracaryum* ought not to be so retained. Some notes which, with our original characters, were not printed by Hemsley, were thought to show that certain genuine species of the former genus nearly agreed in the elevated gynobase, etc., with those that have been referred to *Paracaryum*.

Pinus latisquama Engelm., published in the Gardeners' Chroncle with wrong foliage (that of *P. Ayacahuite*) is here correctly characterized.

Spiranthes of Richard is changed on page 159 to *Speiranthes*. This, although six times repeated, is purely accidental, and was by no means intended as a correction of Richard's latinization of he Greek word concerned, which is that of Virgil and Ovid.

Glyphosperma (*Palmeri*) and *Hemiphylacus* (*latifolius*) are two new genera of *Liliaceæ* from Mexico, the first of the subtribe *Anthericeæ*, the second verging to the *Chlorogaleæ*.

Tradescantia leiandra Torr., is declared to belong to that genus, not to *Zebrina*. A note gives a revision of our species of that genus and of *Commelina*.

The *Gramineæ* have been worked out with much pains-taking, and a good beginning made in the study of the grasses of the Mexican border, while awaiting the long-delayed work of Fournier upon those of Mexico. *Bouteloua* is elaborated in a footnote, and twenty-five species recognized.

A few closing pages are devoted to "Descriptions of some New Western Species" of the United States, the most interesting being two new *Microstyles*, discovered by Mr. Lemmon in southwestern Arizona, and *Montia Howellii*, from Oregon, sent by the brothers Howell, with some anomalous features. There are, of course, one or two new *Eriogona* and an *Allium*.

As to Dr. Palmer's collection, not only are his plants nearly all determined, but also those of a considerable collection made by the late Dr. Schaffner, sent in the year 1880 to the present writer, which has been divided between the Gray herbarium and that of Kew. This collection was from the same district (San Luis Potosi) as Parry and Palmer's collection, whose numbers are also cited in considerable part. A small collection sent by Professor Dugès from farther south (Guanajuato) is also reported on. So that these two Contributions — parts x and xi—are of very great importance in the study of the botany of the northern States of Mexico, a district which is becoming more and more open to American explorers. Dr. Palmer's present collections, and those of his forerunner Dr. Gregg, were made in Nuevo Leon and Coahuila. Chihuahua is now equally accessible; and to the western part of that State, and to adjacent Sonora, which may be reached in time, we look for the most interesting future accessions to the botany of the Mexican frontiers. A. G.

4. Professor Herman Müller. *The Fertilization of Flowers;* translated and edited by D'Arcy W. Thompson, B.A., etc., with a preface by Charles Darwin. With illustrations. London: Macmillan & Co. 1883, pp. 669, 8vo.—The original German edition was of the year 1873, and in this new one, edited as well as translated by Mr. Thompson—turned into good idiomatic English and attractively printed—is incorporated a large mass of the author's recent observations as well as details from his other writings. An added interest is given by the prefatory notice, "full of suggestions, full of kindly, appreciative feeling," which is one of the very last issues of Mr. Darwin's pen. A sadder interest is now given to the volume by the announcement which has just reached us of the death of Dr. Müller, on the 25th of August last. A great, and we judge a premature loss, for though we believe he was not young, he cannot have been old. His earliest published writings of which we have record do not date back of 1850, and the first upon the subject which he had made so fully his own has the date of 1869. Meanwhile, by his most laborious and patient observations, by his great acuteness in interpretation and research, and by his studies of the modifications of insects in relation to flowers, no less than those of flowers to insects, he had placed himself at the head of this curious branch of biology, which was initiated by his countryman, Christian Conrad Sprengel about one hundred years ago, resuscitated and more broadly based in our days upon "the Knight-Darwin law," and the lead in which, since Darwin's death, is restored to Germany mainly by the researches of Hildebrand and Herman Müller.

At this moment there is neither time nor room for more than the announcement of this volume, which forms an encyclopedic text-book upon this subject, and which will be largely used as such in this country, where the way is well prepared for it. The book has full indexes, both of plants and insects concerned, and a full bibliography of 814 entries, filling 32 pages. There are many things upon which one would wish to remark, and there is one little omission. "The observations of Darwin (1861), and Asa Gray (1862)," are referred to through the bibliography, but without noting that the first did not anticipate, and that the second did anticipate and announce in this Journal, Müller's later discovery of the mode of fertilization of *Cypripedium*, in 1868, which he describes on page 339. The curious modification of the papilionaceous flower in *Apios* is unnoticed. A. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *American Association for the Advancement of Science at Minneapolis.*—The meeting of the Association at Minneapolis, under the presidency of Professor C. A. YOUNG, of Princeton, closed on Wednesday, August 22d. The number of members present was large, but less than at Montreal and Boston, for the obvious reason that the place was less central for workers in science. They were most generously treated by the citizens of Minneapolis.

The sections which had the largest number of papers were the geological and archæological, and the geological subject which absorbed the most time and attention was that of the phenomena of the Glacial era and later Quaternary in America.

Dr. J. W. DAWSON, in his address as retiring president, "On some Unsolved Problems in Geology," spoke first of some of the peculiarities and divisions of the Archæan rocks, taking, in his few words, the side of the probable pre-Cambrian age of the Taconic system, and adding beyond, "after the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves in the Cambrian," etc. Dr. Dawson next spoke briefly of the bearing of geological facts on the theory of evolution, of certain new views as to the origin of coal from cryptogamous vegetation, and of the events of the Glacial era.

The "unsolved problems" are treated in the address without much labored argument or much effort to solve them beyond what is contained in the expression of an opinion. For example, on the Taconic question the remark about "uncertain forms" or fossils in its beds, is the best fact on the subject stated, there being no reference to the recent investigations of the rocks of the Taconic Range, and their stratigraphical relations to associated beds containing fossils that are not "uncertain," found both in Vermont and in Dutchess County, New York, or the good fossils in the Poughkeepsie Taconic slates.

The address of Professor ROWLAND, vice-President of the Chemical Section, was an admirable "plea for true science." One sentiment, not new, but true, is here cited to aid in its dissemination. He says, with reference to the government duty on foreign books and periodicals, "We call this a free country and yet it is the only one in which there is a direct tax on the pursuit of science. The low state of pure science in the country may possibly be attributed to the youth of the country; but a direct tax to prevent the growth of our country in that subject cannot be looked upon as other than a deep disgrace." In the science of physics "no books above elementary ones have ever been published, or are likely to be published, in this country; and yet every teacher in physics must have them, not only in the college library but on his own shelves, and must pay the government of this country to allow him to use a portion of his small salary to buy that which is to do good to the whole country." "One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off."

Professor W. A. **Rogers**, vice-President of the Section of Mathematics and Astronomy, treated in his able address of the German survey of the Northern Heavens.

The address of the vice-President of the Geological Section, Professor C. H. **Hitchcock**, was on "the Early History of the North American Continent." Professor Hitchcock presented views with reference to the origin and subdivisions of the early crystalline rocks, and the occurrence of many "oval" isolated Archæan areas in eastern North America, and put forth his latest conclusions as to the *igneous* derivation of the Laurentian areas, arrived at after a visit to the volcanic Hawaiian Islands, he holding, in other words, that "the first land consisted of volcanic islands."

A lecture was delivered also by Professor E. D. **Cope** before the association in general session, on the evidence for evolution in the history of the extinct mammalia. It was a valuable discourse by one who had, to a very large extent, gathered his own facts. He observes that since 1860 the number of known American species of mammals has increased from 250 to nearly 2000.

These addresses are published in full in Science, numbers 27 to 31.

On the subject of the Quaternary the following are some of the facts presented.

Professor G. F. **Wright** treated of the southern limit of the glacier, giving, first, credit to earlier investigators, and speaking of Professor W. Upham as the first to survey the whole of the part of the line which lies between Cape Cod and Brooklyn, and Professors Cook and Smock, that across New Jersey. The historical facts presented are given in this Journal in various notices and in papers by Mr. Upham, Professor Smock, Professor Wright, Professor Chamberlin and others. Professor Wright added the following to the account of the southern limit in his recent paper; that the line through Indiana grazes again the edge of Kentucky opposite Madison, and reaches its southernmost point near Charleston; then bears north through Scott and Bartholomew Counties and follows the line between the latter and Brown County to the northeast corner of Brown; then turns southwest to the northeast corner of Monroe; then bears northward for ten miles to near Martinsville, in Morgan County; then passes west and south, diagonally, through Owen and Green Counties, and to Knox in Harrison township, in Knox County.

Professor CHAMBERLIN, who also early surveyed the line from Long Island westward, presented an important paper on "the Terminal moraines of the second Glacial epoch."

Professor I. C. WHITE treated of the high terraces of western Pennsylvania, a subject discussed by Professor J. J. Stevenson in this Journal for 1878 (III, xv, 245), and said that the facts were wholly in accordance with the view brought out by Professor Wright as to the existence, in the Glacial era, of an ice-dam 500 to 600 feet high across the Ohio valley at Cincinnati. The five several terraces at Morgantown are 30, 75, 175, 200 and 275 feet above the river. The deposits of the upper terrace "consist almost entirely of clays and fine sandy material, with few bowlders." The terrace and its clays have been traced, at the "same absolute level," on other tributaries of the Monongahela, namely, Decker's, Dunkard, Whitely, Muddy and Ten Mile Creeks. The dam set back the waters of the Kanawha and filled the Mud-Guyandotte arm of the valley; and afterward its removal left the valley high and dry. These and other facts were regarded as proving the truth of Professor Wright's hypothesis. The height of dam at Cincinnati required by the facts, according to Professor White, is about 625 feet, or 1050 to 1075 above tide-level.

Professor W. UPHAM presented a paper on the Minnesota valley in the Ice Age in which he gave his reasons for holding that the existence of the great flooded Winnipeg lake—Lake Agassiz, as he calls it—was due to a damming of the waters of the region by a barrier of glacier ice on its north margin preventing the waters from flowing into Hudson's bay, their present course. He appeals to two epochs of glaciation, and remarks that in the first the area was covered with drift, mostly the unstratified; during the interval between the two the pre-glacial valley of the region was deepened by the drainage; during the later Glacial epoch the till only partially blocked up the river-course along its greater part, and portions that were obstructed were channeled anew from the mouth of the Minnesota to New Ulm. As long as the ice-barrier existed on the north the waters poured from "Lake Agassiz" down the Minnesota to the Mississippi, at first depositing drift, but later, when the waters carried little drift material, excavating the existing river-channel through the deposited drift. This view makes the deposits of the valley to have been formed in one era and the channel, between the terraces, in another following. He holds also that the water of Lake Superior may have been held back in a similar way, at a level about 500 feet above its present height, and explains certain facts connected with the St. Croix River on this supposition.

Another paper by Professor Upham treated of the "Changes in the currents of the ice of the last Glacial epoch in eastern Minnesota."

Dr. **Newberry** presented a paper "On the eroding power of ice," attributing to it far greater effects than are conceded by most writers on the subject, and making it, as he had done before, the excavating agent of the basins of the Great Lakes. He also spoke, with a good array of facts, on the reality of the age of ice, and the wide extension of the great northern glacier.

A paper by Professor G. H. **Stone**, "On the Kame rivers of Maine," was read by Professor Upham, in which the origin of kame deposits was discussed, whether super-glacial or sub-glacial, and favoring the former view. He made the important statement that in Maine no southward motion in the glacier would have been possible after the ice had thinned down to 500 feet on account of transverse ranges of hills.

Professor Upham read a paper by Miss F. E. **Babbitt**, of Minnesota, "On Glacial Man in Central Minnesota." The evidence consists in the discovery of "rudely worked quartzes." They were first found by Professor N. H. Winchell in the top portion of certain terraces in Central Minnesota about 100 miles northwest of St. Paul, in the village of Little Falls, Morrison County. Recently similar discoveries have been made in the same region by Miss Babbitt in beds of the terraces below the upper. Many hundreds of chips were found in a small area, none worn, although the stony material of the terrace was well rounded; and every freshet turned out other specimens. The specimens come from a well limited layer, a few inches thick, 12 to 15 feet below the top of the terrace.

In the discussion which followed the reading of the paper, Professor F. W. Putnam stated that some of the specimens exhibited in illustration of the paper were unquestionably of human workmanship.

Other papers on related subjects are those of W. J. **McGee**, "On the formation of Glacial Cañons"; of **Julius Pohlman**, "On the history of the Niagara River"; of W. **McAdams**, "On the lœss and glacial clays of Alton, Illinois.

The following is a list of the papers accepted for reading at the sessions of the Association:

List of Papers accepted for Reading.

Section A, Mathematics and Astronomy.

Edward S. Holden: The total solar eclipse of May 6, 1883.

W. A. **Rogers**: A new method of investigating the flexure corrections of a meridian circle; On an improved method of producing a dark-field illumination of lines ruled upon glass.

E. W. **Hyde**: The calculus of direction and position.

S. C. **Chandler, Jr.**: Results of tests with the almacantar in time and latitude; Investigations of light variations of Sawyer's variables.

J. B. **Eastman**: Internal contacts in transits of inferior planets.

G. W. **Hough**; Physical phenomena on the planet Jupiter; The rotation of domes.

J. **Janssen**: French observations of the total eclipse of May 6th, 1883.

O. S. **Westcott**: Some hitherto undeveloped properties of squares.

E. F. **Sawyer**: On the light variations of *T. Monocerotis.*

Edgar Frisbie: Orbit of the great comet of 1882.

J. K. Rees: Observations on the transit of Venus made at Columbia College, ew York City: Description of new observatory of Columbia College.

C. M. Woodward: Descriptive geometry applied to the general ellipsoid.

J. Burkitt Webb: Conic sections in descriptive geometry; Descriptive geomet- cal treatment of surfaces of the second degree; The kinematics of rotary pumps.

C. A. Young: Some observations on Uranus.

D. P. Todd: Methods of observing eclipses of Jupiter's satellites.

Samuel Emerson: Standard time-pointer and a time longitude dial; System of gebraic geometry.

J. M. Batchelder: Tidal observations on soundings distant from shore.

Section B, Physics.

W. A. Rogers: Definitive determination of the relation: Imperial yard ·3·37030=the meter of the archives.

E. S. Morse: On the utilization of the Sun's rays for warming and ventilating artments.

H. S. Carhart: The magnetophone, or the modification of the magnetic field the rotation of a perforated metallic disc.

A. E. Dolbear: The static telephone.

F. E. Nipher: Magnetic survey of Missouri; Plan for a state weather service.

T. C. Mendenhall: A method of distributing weather forecasts by means of ilways.

P. R. Hoyt: The tornado at Racine, May 18, 1883.

J. H. Frick: The tornado of May 18th.

C. E. Fritts: On a new form of selenium cell, and some remarkable electrical scoveries made by its use.

H. T. Eddy: On the kinetic theory of the specific heat of solids; a kinetic eory of melting and boiling; An extension of the theorem of the virial to rotary cillation.

B. F. Thomas: A method for the calibration of a galvanometer; A method of termining the center of gravity of a mass; Two forms of apparatus for Boyle's w; A new heliostat.

Samuel Hart: Natural snow-balls or snow rollers.

Gustavus Hinrichs: Remarks on the tracings of self-registering instruments d the value of the S. S. indications for Iowa in June and July, 1883.

Section C, Chemistry.

R. B. Warder: Suggestions for computing the speed of chemical reactions.

J. W. Langley and **C. K. McGee**: On the sub-aqueous dissociation of certain lts.

C. F. Mabery and **H. H. Nicholson**: On gamma-dichlordibromopropionic and mma-dichlorbromacrylic acids.

C. F. Mabery and **Rachel Lloyd**: On the formation and constitution of chlor-romacrylic acid.

C. F. Mabery and **G. M. Palmer**: On orthoiodtoluolsulfonic acid.

H. W. Wiley: American butters and their adulterations.

Clifford Richardson: The composition of American wheat and corn; Sotol, xican forage plant.

E. L. Sturtevant: Twelve months of lysimeter record at the New York Agri-ltural Experiment Station.

W. H. Seamen: New forms of burettes.

C. Leo Mees: Estimation of carbon and nitrogen in organic compounds.

Section D, Mechanical Science.

W. A. Rogers: A method of testing long plane surfaces, applicable to the gnment of planer-beds, lathe-beds, heavy shafting, etc.

R. H. Thurston: The commercial and dynamic efficiencies of the steam-engine; ntrifugal action in turbines.

T. R. Baker: A comparison of terra-cotta lumber with some other building materials.

C. M. Woodward: Descriptive geometry applied to the general ellipsoid; Velocity of the piston of a crank engine.

J. B. Webb: Improvements in shaping machines; Regularity of flow in double-cylinder rotary pumps.

Section E, Geology and Geography.

G. F. Wright: Result of explorations of the glacial boundary between New Jersey and Illinois.

T. C. Chamberlin: The terminal moraine west of Ohio.

Warren Upham: The Minnesota valley in the ice age; Changes in the currents of the ice of the last Glacial epoch in eastern Minnesota.

I. C. White: Relation of the glacial dam at Cincinnati to the terraces on the Upper Ohio and its tributaries.

N. H. Winchell: The comparative strength of Minnesota and New England granites; Clay pebbles, with an exhibition of specimens from Princetown, Minnesota.

J. W. Dawson: On Rhizocarps in the Paleozoic period.

James Hall: On the microscopic structure of the test of fossil Brachiopoda.

W. J. McGee: On Glacial canyons.

Julius Pohlman: The life-history of the Niagara River.

H. C. Bolton and A. A. Julien: The singing beach of Manchester, Mass.

A. S. Tiffany: The equivalent of the New York water-lime group developed in Iowa.

Richard Owen: The Earth's orographic framework: its seismology and geology; The "Continental Type" or normal orography and geology of continents.

J. W. Chickering, Jr.: Thermal belts.

Wm. McAdams: A new vertebrate from the St. Louis limestone; Animal remains from the Lœss and Glacial clays.

G. H. Stone: The kame rivers of Maine.

J. D. Dana: Evidences from southern New England against the iceberg theory of the drift.

J. S. Newberry: On the eroding power of ice; The ancient glaciation of North America, its extent, character and teachings; On the genesis and classification of mineral veins.

T. Sterry Hunt: The Pre-Cambrian rocks of the Alps; On the serpentine of Staten Island, New York.

James Macfarlane: The "earthquake" at New Madrid, Mo., in 1811, probably not an earthquake.

E. W. Claypole: On the Hamilton sandstone of middle Pennsylvania; On a large Crustacean from the Catskill group of Pennsylvania; On Rensselaeria and a fossil fish from the Hamilton group of Pennsylvania.

E. D. Cope: On the structure of the skull in Diclonius mirabilis, a Laramie Dinosaurian; On the Trituberculate type of superior molar, and the origin of the Quadrituberculate; On two primitive types of Ungulata.

Wm. McAdams: Animal remains from the Lœss and Glacial clays.

A. A. Julien and H. C. Bolton: The singing beach of Manchester, Mass.

G. F. Kunz: Topaz and associated minerals from Stoneham, Oxford county, Maine; Colored tourmalines and lepidolite crystals from a new American locality; A note on the finding of two American beryls; Andalusite from a new American locality; On a white garnet from near Hull, Canada.

Section F, Biology.

W. G. Farlow: Relations of certain forms of Algæ to disagreeable tastes and odors; The spread of epidemic diseases in plants.

D. S. Kellicott: Psephenus Lecontei; on the external anatomy of the larva.

E. L. Sturtevant: Parallelism of structure of maize and sorghum kernels; Influence of position on seed; Agricultural botany.

D. P. Penhallow: Relation of root and leaf areas; Corn.

E. W. Claypole: Note on the present condition of the box huckleberry, Vacinium brachycerum, in Perry county, Pennsylvania; Notes on the potato beetle nd Hessian fly for 1883.

C. P. Hart: Conscious automatism.

E. S. Morse: Mya arenaria; its changes in Pliocene and prehistoric times; On new plan of museum case.

S. H. Gage: Pharyngeal respiration in the soft-shelled turtule, Aspidonectes pinifer.

W. R. Dudley: An abnormal orchid, Habenaria hyperborea; Origin of the Flora f the central New York lake region.

Miss M. E. Murtfeldt: Periodicity of Sabbacia angularis.

E. P. Howland: The application of nitrous oxide and air to produce Anæshesia, with clinics on animals in an experimental air chamber.

J. M. Coulter: Development of a dandelion flower.

W. J. Beal: Leaves of the Gramineæ with closed sheaths.

J. C. Arthur: A supposed poisonous sea-weed in the lakes of Minnesota.

Joseph F. James: The position of the Compositæ in the natural system.

B. G. Wilder and S. H. Gage: On the use of vaseline to prevent the loss of lcohol from specimen jars.

Herbert Osborn: Note on Phytoptidæ.

E. S. Bastin: A fact bearing upon the evolution of the genus Cypripedium.

C. V. Riley: The Psyllidæ of the United States; Some recent discoveries in eference to Phylloxera.

A. Hyatt: Observations on Cephalopoda.

Section H, Anthropology.

F. W. Putnam: An abnormal human skull from a stone grave in Tennessee; A ew stand for mounting skulls, by E. E. Chick.

J. O. Dorsey: Osage war customs.

Mrs. Erminnie A. Smith: Accidents or mode sides of verbs in the Iroquois ialects; Life among the Mohawks in the Catholic missions of Quebec Province; tudies in the Iroquois concerning the verb *to be* and its substitutes.

S. D. Peet: The correspondence between the prehistoric map of North America nd the system of social development; Game drives among the emblematic ounds; Typical shapes among emblematic mounds: the different attitudes xhibited by the same animal; High places connected with ancient villages; the eligious structures common to villages in prehistoric times; An ancient village f the emblematic Mound Builders; Caches guarded by effigies; Effigies guardg the village, and sacrificial places not far away.

O. T. Mason: The Charnay collection at Washington.

E. S. Morse: Kitchens of the East; Methods of arrow release; In-door games f the Japanese.

Wm. McAdams: The great mounds of Cahokia.

Charles Whittlesey: Metrical standards of the Mound Builders by the method f even divisors.

John Campbell: The Mound Builders identified.

E. P. West: Personal observations of the Missouri River mounds from Omaha St. Louis considered from a geological stand-point. Their invariable association ith the Lœss and Terrace formation.

Miss A. C. Fletcher: Some observations on the laws and privileges of the ens in Indian society; Symbolic earth formation.

Miss F. E. Babbitt: Vestiges of Glacial man in central Minnesota.

J. W. Powell: A classification of the sciences.

Section I, Economic Science and Statistics.

Elizur Wright: Life insurance and self-insurance.

C. S. Mixter: The increase of the colored population of the United States.

H. C. Hovey: Oyster farming in Connecticut waters, with a map of the grounds. pecimens of shells, etc.

C. W. **Smiley**: The German carp and its introduction into the United States.
E. T. **Cox**: Cable cars for city passenger traffic.
Edgar Frisbie: Building associations.
Stephen S. Haight: Health foods.
C. V. **Riley**: Improved methods of spraying fruit and shade trees for protection against leaf-feeding insects.
J. R. **Dodge**: Enhancement of values in agriculture by reason of non-agricultural population.
T. E. **Jefferson**: A new system for the treatment of sewer gas.

The next meeting of the Association will be held in Philadelphia, Professor J. P. **Lesley** was elected president for the year. The vice-presidents appointed are, Professor H. T. **Eddy**, of the Section of Mathematics and Astronomy (A); **John Trowbridge**, Cambridge, Mass., of Physics (B); J. W. **Langley**, Ann Arbor, of Chemistry (C); R. H. **Thurston**, Hoboken, of Mechanical Science (D); N. H. **Winchell**, of Geology and Geography (E); E. D. **Cope**, of Biology (F); T. G. **Wormley**, of Histology and Microscopy (G); E. S. **Morse**, of Anthropology (H); Hon. **John Eaton**, Washington, D. C., of Economic Science and Statistics. Dr. **Alfred Springer**, of Cincinnati, was chosen General Secretary, and F. W. **Putnam** was continued as Permanent Secretary.

2. *British Association.*—The fifty-third annual meeting of the British Association was held at Southport, between the 19th and 27th of September.

OBITUARY.

Professor J. **Reinhard Blum** of Heidelberg, for many years one of the leading mineralogists of Germany, died on the 22d of August last in his 81st year. Professor Blum's most important contribution to science was his treatise on Pseudomorphs published in 1843, with the four supplements bearing the dates of 1847, 1852, 1863, 1879. This subject he developed with great care and thoroughness, and although subsequent study will doubtless carry the subject much further, the works of Blum will never lose their value and the large number of facts he brought together will remain the foundation of future work in this field. In addition to the publication of the volumes mentioned, he also made a large typical collection of pseudomorphs illustrating the subject described; this collection became in 1870 the property of Yale College and is preserved in New Haven. Professor Blum was also the author of a general work on Mineralogy, and of another on Lithology and of various shorter mineralogical papers. He was a man of most genial and friendly character and his many pupils who listened to his teaching between 1828 and 1877, remember with pleasure the instruction and encouragement which they received from him.

William A. Norton, Professor of Civil Engineering in the Sheffield Scientific School of Yale College, and the author of various papers in this Journal, and of works on Astronomy and Physics, died on the 21st of September, in his 73d year. A fuller notice is deferred to another number.

THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XXXVI.—*Spectroscopic Notes;* by Professor C. A. YOUNG, Princeton, N. J.

FOR the past few months I have been examining the spectra of sun-spots with great care, and with an instrument of high dispersion.

The spectroscope employed consists of a comet-seeker of five inches aperture and about forty-eight inches focal length, used both as collimator and view-telescope after Littrow's method, the slit and diagonal eye-piece being as close together as it is convenient to place them. A small spot of black paper, about three-tenths of an inch in diameter, is cemented to the center of the object-glass (as suggested by my colleague, Professor Brackett, in a note published in this Journal, July, 1882), and entirely destroys the internal reflections which would otherwise most seriously interfere with vision.

The dispersion is obtained by one of Professor Rowland's nagnificent gratings on a speculum-metal plane, with a ruled urface three and one-half inches by five; 14,000 lines to the nch. The slit and eye-piece of the telescope are so placed that he line joining them is parallel to the lines of the ruling. An nstrument of this sort is incomparably more convenient than ne in which the collimator and view-telescope are separate, hough of course, on account of the inclination of the visual ays to the axis of the object-glass, there is a little aberration, nd the *maximissimum* of definition is not quite reached. There s no difficulty, however, in seeing easily the duplicity of the *b*'s, E, and other similar tests, with the instrument thus arranged. The spectroscope is mounted upon a strong plank, stiffly braced, and this is attached by powerful ring-clamps to the tail-piece of the twenty-three-inch equatorial of the Halsted observatory, so that the image of the sun falls directly upon the slit.

The detailed examination of the spot-spectra has been thus far confined mainly to a few limited regions in the neighborhood of C, D and *b*.

With the high dispersive power employed, the widening and "winging" of the heavier lines of the spectrum is not well seen, not nearly so well as with a single prism spectroscope. All diffuse shadings disappear much in the same way as the naked eye markings on the moon's face vanish in a powerful telescope—to be replaced by others more minute but not less interesting. In a few spots, however, the broadening of the D's and the reversal and occasional "lumping" of C has been noticeable even with this high dispersion. But the most striking result is that in certain regions the spectrum of the spot-nucleus, instead of appearing as a mere continuous shade, crossed here and there by markings dark and light, is resolved into a countless number of lines, exceedingly fine and closely packed, interrupted frequently between E and F (and occasionally below E) by lines as bright as the spectrum outside the spot. These bright lines, so far as the eye can determine, may be either real lines *superposed*, or merely vacancies left in the shading of fine dark lines, since they are not sensibly brighter than the ordinary background of the surrounding spectrum.

The darker and more intense the spot, the more distinctly the fine lines come out, both the bright and the dark; and so far as I have been able to make out yet, there is no difference as regards these fine lines between one spot and another. I have never yet seen any evidence of displacement in them due to motion, no "lumpiness" nor want of smoothness in them.

When seeing is at the best, and everything favorable, close attention enables one to trace nearly all these lines out beyond the spot and its penumbra. But they are so exceedingly faint on the sun's general surface that usually they cannot be detected outside the spot spectrum. This resolution of the spot-spectrum into a congeries of fine lines is most easily made out in the green and blue. Near D, and below it, it is much more difficult to see, and I am not even quite sure that this structure still exists in the regions around C and below it. Here, in the red, even with the highest dispersion and under the most favorable circumstances of vision, the spot-spectrum appears simply as a continuous shade, crossed here and there by widened and darkened lines, which, however, are very few and far between as compared with the number of such lines in the higher regions. Of course the resolution of the spot-spectrum into lines tends to indicate that the absorption which darkens the center of a sun-spot is produced, not by granules of solid or liquid matter, but by matter in the gaseous form, and it becomes interesting to inquire what substances are capable of producing such a spectrum, and under what conditions. As to the fineness and number of the lines, it may be noted that in the region included between b_1 and b_4 the single lines appear to be each about half as wide as the components of b_2, and are separated by an interval about one-third as great. The whole number between b_1 and b_4 must be over a hundred, though they are of course very difficult to count with accuracy. They are a little wider in the middle of the spot-spectrum, in fact spindle-shaped, running out into extremely fine threads where they pass into the penumbra, and in my instrument they seem to be a little more hardly and sharply defined on the upper (more refrangible) edge than on the lower.

The bright lines, of which there are six between b_1 and b_4, are generally about as wide as the interspace between the components of b_2. They are sharply defined at both edges, and no brighter at the center than at the edge, a fact which rather bears in favor of the idea that they are merely interruptions in the dark line series, and not really superposed bright lines. Just above b_4 (at λ 5162·3) there is a very conspicuous one, which is also noticeable enough in the ordinary solar spectrum. Attention has indeed been frequently called to it long since by other observers. Below E these bright lines are rare. Higher up in the spectrum, between F and G, they become very numerous.

I have also made a considerable number of observations upon prominences with the nine and one-half inch equatorial and its own spectroscope. There have been lately numerous very fine exhibitions, especially in connection with the spots. The number of lines reversed in the spectrum of the chromosphere has at times been very great, far exceeding the number observed and catalogued in 1872, but I have not been able to detect a single *new* one below C, though the two mentioned in my catalogue have been seen almost continuously. On two occasions—July 31st and August 1st—a new line above H (λ 3884±2) was conspicuously visible for an hour or two each time, during a specially vigorous eruption of the prominences associated with the great spot which was then just passing off the limb. This line was seen easily without the aid of any fluorescent eye-piece, and I am satisfied that on a photographic plate it would have been more brilliant than either H or K. I could not determine its position within one or two units on account of the difficulty of identifying the numberless fine lines around it.

With the widened slit it showed clearly the form of the lower part of the prominence, but not the upper. It was almost precisely imitated by two new lines at λ 4092 and 4026; and the catalogue-lines 4077 and 3990 resembled it also. On the other hand, *h*, H and K showed the higher parts of the prominence as well as the lower, while the lines at 4045 and 3970 were exceedingly fine and smooth, without knottiness or structure.

On August 1st, at 2^h 58^m local time ($=7^h$ 57^m Greenwich time), the intensity of the chromosphere spectrum was very remarkable, the bright lines more vivid and numerous than I remember ever to have seen them before. Between this time and 3.12 a prominence was shot up in fragments of flame to an elevation of over 120,000 miles. It will be interesting to learn whether any corresponding magnetic twitch appears on the magnetometer records.

Princeton, N. J., Sept. 10, 1883.

ART. XXXVII.—*On Meteoric Iron from near Dalton, Whitfie County, Georgia*; by CHARLES UPHAM SHEPARD, SR.

WHETHER the mass here described is of identical origin with that found in 1877, and described by Mr. W. Earl Hidden in vol. xxi, No. 124, p. 287, of this Journal, is not quite certain. The circumstances connected with the finding of the present mass are detailed in the following letter of H. C. Hamilton, Esq., of Dalton, dated Oct. 18th, 1882, addressed to Major E. Willis of Charleston, S. C.:

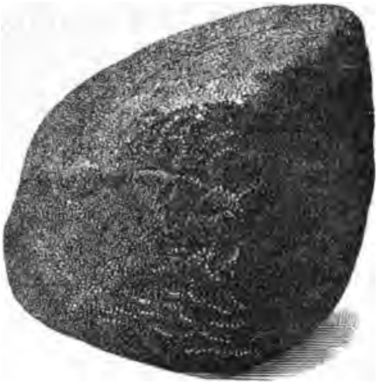
"*Sir*—Yours of the 14th inst. from Professor Charles U. Shepard, Jr., in regard to the meteorite is received. The meteorite was found some time in the year 1879 by Francis M. Anderson, on his farm, on lot 109 in the 10th district and 3d section of Whitfield county, Georgia, about 14 miles northeast of Dalton. It was discovered while plowing, on the west side of a ridge, near its base. The ridge runs north and south, and the furrows cut east and west. It was lying with its apex upward, and buried about six inches below the surface of the ground.

Some time during the fall of 1860 an unusual atmospheric phenomenon occurred in the region. A bright light shot across the heavens, followed by a loud report, creating great alarm among the people, many of whom supposed the end of the world had arrived.

A large mass of iron, supposed to be a meteorite, was found half a mile from this one, about the year 1862. It was sent

Cleveland, Tennessee, where it appears to have been lost ght of, as nothing further can be ascertained respecting it."

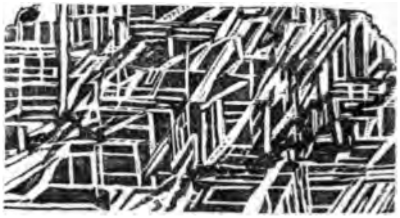
The mass now described belongs to the meteoric collection f C. U. Shepard, Jr., of Charleston, S. C. Its weight is 117 ounds.



The wood-cut shows its general shape, which is somewhat t of a pear, whose apex is slightly trihedral. The surface is arly black, and very little oxidated. It is destitute of deep lentations; and presents a surface with only faint, wave-like pressions.

It is more easily divided with the saw than most other irons, ing to the absence of pyritic veins and kernels. When oken across a thickness of $\frac{1}{8}$th of an inch, it presents a coarse, t highly uniform, granular structure, and shows a cleavage the individuals, much resembling that of the Braunau iron. s polished surfaces are almost perfectly homogeneous, being olly free from amygdaloidal inclosures. At places within inch of the outside however, a few very thin, black, thread-e veins occur, apparently the products of shrinkage in the ocess of cooling. The substance of these veins is partly the ides of iron, with which is intermingled a trace of chlorine, e latter probably derived by infiltration from the soil. The liquescence of the polished surfaces is confined to these black ams.

The Widman figures present a rather remarkable feature. the annexed wood-cut they are copied from an actual pression derived from an etched surface of the iron, with the nission only of the finer lines that enclose the white bars d traverse the shaded parallelograms and triangles. The fine es are not introduced owing to the difficulty of their proper presentation on wood. The peculiarity of the pattern consists, the wood-cut shows, in the tendency in one series of bars to cross each other at right angles, while in a second series, less uniform in width, they pass diagonally (in descending) from right to left across the shaded spaces.



The shaded portions are in no way different in composition or structure from those which are in white, as may be seen by receiving the light from them on the polished metallic surfaces at a slightly different angle. This changeability doubtless depends upon a crystalline structure analogous to that producing a similar effect in numerous crystalline minerals, particularly in the family of feldspars.

On etched surfaces, the schreibersite shows in exceedingly thin and nearly straight continuous lines, though they are occasionally interrupted at short intervals, when they resemble the markings on telegraphic ribbands. The continuous lines sometimes swell into triangular or polygonal enlargements, forming a string of nearly disconnected beads, as represented by G. Rose in the Zacatecas iron. These beads he denominates crystals.

The specific gravity of the iron is 7·986, a density somewhat higher than common in meteoric irons, but not unexpected in one like the present, destitute of sulphur and containing less phosphorus than usual in these bodies.

The analysis of C. U. Shepard, Jr., gave

Iron	94·66
Nickel	4·80
Cobalt	0·34
	99·80

With phosphorus, chromium and manganese in traces.

It is to be regretted that we are unable to decide whether this meteorite belongs to the same fall with the 13-pound mass described by Mr. Hidden. The two were found at different times, several miles asunder, and were totally unlike in the character of their surfaces. Besides, the smaller abounded in chlorous deliquescence; and it appears to have been easily separated by cleavage, suggesting a longer contact with the soil.

ART. XXXVIII.—*Notice of Corundum Gems in the Himalaya region of India*; by CHARLES UPHAM SHEPARD, SR.

THE discovery of a remarkable locality of the sapphire and ruby in the Himalaya Mountains has been communicated to me by Rev. M. B. Carleton of Keoloo, India, a former pupil of mine, and graduate in 1877 of Amherst. He derived his information from the following descriptive note of Mr. Grahame Young of Kulu.

"There are two versions of the discovery of the corundum deposits at Sungchang in Zanskar, one being that they were exposed by a hill-side slipping, the other that they were discovered by hunters. Their value was so little known that the villagers bartered them for a trifle to Lahouli traders, who in their turn vainly endeavored to exchange them for grain in Kulu. On their value becoming known, there was a rush of jewelers from Delhi and other places; and they speedily rose to 100 rs. per tola = about £20 stg. per oz., for good specimens, at which rate they have remained; at present none are to be had, all the stock brought down has been sold, and the mine is strictly guarded by one of the Maharajah's Dogra regiments.

So far as I can learn, the matrix is a schistose or slaty rock. The vein consists of

I. Ordinary quartz crystals, some very large.

II. A few crystals of amethyst.

III. Deep blue corundum of a beautiful water, very rough externally, no crystal more than 4 inches long; sp. gr. 3·985.

IV. Corundum, sapphire-colored only in the middle, shading lighter until both base and apex are perfectly limpid.

V. Perfectly limpid corundum.

VI. Black corundum.

VII. Opaque white corundum, sapphire tinge in places, small black crystals (probably tourmaline) imbedded. All the above are beautifully crystallized, apex very acute.

VIII. Massive corundum, both black and opaque white.

IX. Chlorite, crystals imperfect.

X. A little magnetite.

The Maharajah has recently released from prison and largely rewarded two native hunters, who had been imprisoned for dealing in sapphire, on condition of their showing him two other deposits, one of *blue* and the other of *red* corundum. I have no information regarding these deposits.* A small fragment of the red corundum has, however, found its way to

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* And up to this time, May 22, 1883, I have heard nothing further concerning them.—W. B. CARLETON.

Kulu; it is true oriental ruby, perfectly clear, and of a beautiful water.

The facts I have collected regarding the first discovered deposit are derived from an examination I made of about an hundred weight of the crystals; their owner would not allow me to apply any tests, but I used a compound lens magnifying 30 diameters. A. GRAHAME YOUNG.

Kulu, Aug. 8, 1882."

The discovery has the greater interest for us owing to certain analogies in the mode of occurrence and some other particulars between the crystals found in India and those produced at several of the American localities. These resemblances are the most striking between the Indian specimens and those of Laurens district, South Carolina. At this latter place the crystals are enclosed in a highly micaceous schist as well as found loose in the contiguous soil; they are very abundant: of the same size, surface and form as those described by Mr. Young; possess a similar coloration except with greatly inferior tints of blue and red; but are traversed by abundant cleavage flaws and destitute of transparency.

The Pelham, Massachusetts, corundum, which, however, is by no means abundant, closely resembles that of Laurens, except that it is imbedded in a scaly biotite intermingled with vermiculite and ripidolite, with occasional inclosures of anorthite seams.

The crystals of the Burke and Iredel county region of North Carolina, though considerably different, still remind us of those from Laurens. So far as I know they are only loose in the soil. They vary from one to four inches in length, are generally pyramidal in form and coated by a glaze of gray muscovite, as though they had originated in mica-slate. Numbers of them, and generally of larger size, are hexagonal prisms, and thickly drused over with short implanted crystals of white margarite. Indeed, nearly all the crystals from this region, when broken across, exhibit an exterior annulus of this species, while the interior is distinctly stellated; and when the surface is polished a copper-colored chatoyement is sometimes presented. The colors of the Iredal crystals are less intense than those of Laurens or Pelham.

The only analogy between the Indian crystals and the sapphire and ruby of Macon and Clay counties, of North Carolina, consists in the association of ripidolite with both.

The resemblances here pointed out appear to me sufficiently important to encourage the expectation that valuable corundum gems may yet be found in the United States.

New Haven, July 19, 1883.

ART. XXXIX.—*Phenomena of the Glacial and Champlain periods about the mouth of the Connecticut valley—that is, in the New Haven region;* by JAMES D. DANA.

THE Glacial phenomena over the New Haven region have many points of special interest, an important part of which arise from the fact that the ice of northern New England which had moved down the Connecticut valley or trough, here left the mainland for a passage through Long Island Sound. The subject includes therefore a discussion of the relations between the part of the glacier to the north and that over the Sound and beyond, and suggests questions as to—

(1) The thickness of the glacier at this southern end of the trough, and the pitch along the trough.

(2) The fact or not of two directions of movement in the ice —a *lower* or *trough stream* south-southwestward, and an *upper stream* southeastward; and, if two, as to their being simultaneous or not.

(3) Any mixing of drift materials from the two streams.

(4) The direction of movement after escaping from the confines of the Connecticut trough and entering the east-and-west trough of the Sound.

(5) Any effects in the New Haven region consequent on a change of direction in the glacier-movement.

The phenomena also of the early Champlain period, when the floods from the dissolving glacier were pouring down the valley and made use of New Haven Bay for the outlet of part of the waters—repeating the drainage conditions that existed long before in Triassic time—have a wide bearing, especially those connected with—

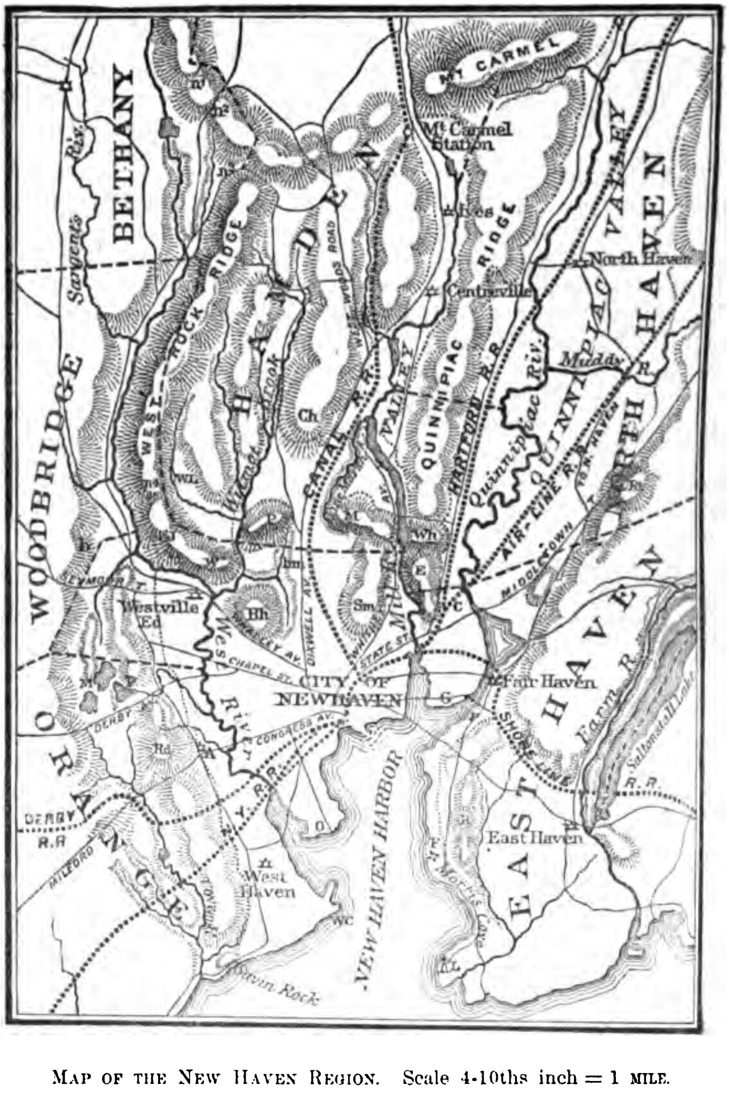
(1) The structure and seaward slope of the New Haven terrace-formation, or the deposits of stratified drift which underlie the broad New Haven plain.

(2) The occurrence, over the flood-made fluvial deposits of the plain, of more than forty isolated basin-shaped cavities called "kettle-holes."

(3) The existence also of two broad north-and-south depressions in the plain, each over a mile long, which look like deserted river channels.

(4) The apparent connection of these depressions and the "kettle-holes" with a former system of drainage, characteristic of the Glacial era.

(5) The existence of a terrace on the bay, testifying to the sea-level during the era of the flood.



Map of the New Haven Region. Scale 4-10ths inch = 1 mile.

Explanations.—A, Allingtown. B, Beacon Hill. Bh. Beaver Hills. Ch, Cherry Hill. E, East Rock range, consisting of East Rock to the north, next south Indian Head, and further south Snake Rock. Ed, Edgewood. F, Fort Hale. *F*, Ferry Point, or Red Rock, on the Quinnipiac. G, Chapel Street. J, Judges Cave, on the West Rock ridge. L, Old Light House. M, Mill Rock. M P, Maltby Park. O, Oyster Point. P, Pine Rock. Rd, Round Hill. Rt, Rabbit or Peter's Rock. Sm, Sachem's ridge. T, Turnpike: also Tomlinson's bridge, across the head of New Haven Bay. V, Whitneyville. W, West Rock, the south end of West Rock ridge. W C, West Cape, or West Haven Point. Wh, Whitney Peak. W L, Wintergreen Lake. *b*, 1200 ton bowlder in Woodbridge. *bm*, Beaver Pond Meadows. *n*1, *n*2, *n*3, *n*4, different notches in the West Rock ridge; *n*1, *n*2, the upper and lower Bethany Notches; *n*3, the Hamden Notch; *n*4, the Wintergreen Notch.

Some of these points were discussed by me in 1870, in a aper on "the Geology of the New Haven Region," published y the Connecticut Academy of Sciences in the second volume f its Transactions. But since that time many new facts have ome to light, sustaining some of the conclusions then presented nd requiring modifications of others, besides affording fuller llustrations of all the various characteristics of the plain. urther, in the interval which has elapsed, a topographical urvey and chart of the region have been made under the uspices of the Coast Survey and the special superintendence f Mr. R. M. Bache, assistant U. S. Coast Survey, laying down he features of the surface with great accuracy, and indicating he level over the whole area by 20-foot contour lines. In my ndeavor to secure such a survey, I had especially in view its se as a basis for geological investigations and reports; and it ulfills well the purpose. Moreover, it has brought to view new acts, and given greater precision to our knowledge of topographical details. A review of the subject with the aid of all he additional light obtained is therefore needed.

On the preceding map, the *New Haven region* comprises all of he area south of the trap-and-sandstone range of Mt. Carmel, nd east of the range of hills of metamorphic schists which ake its western border through the towns of Orange, Woodridge and Bethany.

The *western* border region has a rather abrupt eastern front, nd rises in height from 100 feet above mean-tide level on the uth to 650 feet on the north. This western border was the estern margin of the deep Connecticut trough and estuary of riassico-Jurassic time, all the rocks of the region eastward f it being trap and sandstone of that era; and it was the occaon of important results also in the Glacial and Champlain eriods. South of Westville it is a *double* ridge—that of a roken-backed anticlinal—a shallow valley of ledges and arshes (one-fourth to three-fourths of a mile wide) separating n eastern and a western range of hills.*

The outline of the flood-made terrace-formation is indicated n the map by a dotted line, at the base of the hills. As is een, the three principal valleys and former flood-ways of the egion—West River, Mill River and the Quinnipiac—open outhward into the broad New Haven plain. The Quinnipiac alley is separated from that of Mill River by the Quinnipiac idge, made of sandstone hills 100 to 250 feet in height, with East Rock ridge, 362 feet in greatest height, at its south end; nd Mill River valley from that of West River, by the "West Rock Ridge" of trap (doleryte) 400 to 600 feet in height, ogether with the various sandstone hills that lie to the east of it.

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* The valley is called (from one portion of it at M on the map) Maltby Park alley. See also the map on page 358.

On the eastern margin of the region the hills are of trap and sandstone; the largest of the trap ridges constitutes the western border of Lake Saltonstall.

After this introduction, I will take up, first, the phenomena of the Glacial period, discussing the points in the following order:

1. The fact of a southward (S. by W.) movement in the *lower* ice of the glacier along the Connecticut valley, and the angle of slope of the surface of the ice in the direction of the valley.
2. The fact of a *southeastward* movement (S.S.E. to S.E.) in the *upper* ice.
3. The correlations of the two movements as to time and as to drift depositions.
4. The direction of movement over Long Island Sound.
5. The drift deposits, and the probable effects of the wrenching attending the change of direction which they exhibit.

I. GLACIAL PHENOMENA.

1. *The fact of a Connecticut valley movement and the probable surface-slope of the ice in the direction of the valley.*

a. The fact of the valley movement.—The fact of the valley movement I pointed out in 1871,* basing the conclusions on (1) observations in Massachusetts, by Professor Edward Hitchcock, published in the Massachusetts Geological Report (1841). (2) observations in Vermont, from the Vermont Geological Report (1861), made chiefly by Professor C. H. Hitchcock, (3) a map of New Hampshire by the latter, giving the directions of glacial scratches, published in advance of the final Report on the Geology of that State, and (4) on my own investigations. The facts supplied by Professor Edward Hitchcock, the earliest careful investigator of the subject on the continent, were sufficient alone to establish the truth announced: and he recognized it, but made the ice mainly that of icebergs. The same conclusion is presented in the Vermont Report. But in the New Hampshire Report, published in 1878, the movement is made by Professor C. H. Hitchcock a *glacier* movement.

The fact *is proved by the glacial scratches* All observations from New Haven northward to Windsor in Vermont, 140 miles, give, with rare exceptions, courses between S. and S. 20° W. In the upper part of the valley, north of Windsor to Wells River, 60 miles, both southward and southeastward directions occur.

It *is proved also by the drift transportation.* Of the drift material in the New Haven region, more than 99 per cent, including bowlders innumerable, from one ton to 1200 tons in weight, were derived from the trap and sandstone *of the valley*, that is, were taken up in the valley and transported along the valley. The proof is hence positive that the transporting agent moved down the valley, gathering the stones and earth by the way, and, sooner or later, depositing also by the way. The mean course of the Connecticut valley in Connecticut, from the Massachusetts boundary to New Haven bay, is S. 17° W., and this is about the mean course of the scratches and drift.

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* This Journal, II, li, 233.

b. Directions of the bottom movement over the New Haven region; the large amount of WESTING *along the western border and beyond over Orange and Milford.*

The glacial scratches in the eastern part of the New Haven region, over East Haven, about the various opened quarries, indicate a movement in the direction S. 13° W. The usual trend of the sandstone ridges of East Haven is about the same, showing that they owe their forms to glacier abrasion. Sachem's Ridge (Sm on the map), made into a ridge by the ploughing glacier,* because of the protecting Mill Rock to the north, has a trend of S. 11½° W. In Hamden, farther north, the "Quinnipiac Ridge" (similarly carved out by the glacier because under the lee of Mt. Carmel) has the mean course S. 16° W. The valleys and sandstone ridges west of this ridge have nearly the course of the West Rock ridge to the west of them, or S. 26° W.† Along the western border of the New Haven region, in Orange, the course at the many scratched ledges is S. 26°–38° W., and mostly S. 33° W. But it varies over Maltby Park, (M on map) to S. 45° W. and S. 56° W. owing to local conditions.

The large amount of westing on the western border was pointed out in my paper on the New Haven region of 1870; but I have now the additional fact that the course S. 33° W. occurs to the westward of this border over Orange and Milford, for half a dozen miles from the New Haven region, or nearly to the Housatonic and Naugatuck valleys, showing that the southwestward movement had a wide westward extension. The fact points, I suppose, only to near uniformity of direction in the low and broad valleys and ridges of the surface; and proves that the topography, notwithstanding the fact that the rocks are fragile slates, was of pre-Glacial origin.‡

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* Ploughing deeply over the soft sandstone formation, but feebly abrading over the trap.

† An exception as to westing occurs two miles north of Westville in the narrow West River valley, where the course of grooving over the slate rocks is South; an exceptional course evidently due to the form of the surface at the place.

‡ Many variations in the directions of glacial scratches are due simply to the varying slopes in the land-surface underneath. The variations usually found about a knoll are an example on a small scale. Only when the slopes of the land-surface and upper glacier-surface coincide in compass-course, will the scratches have the direction of the glacier-flow. If these surfaces are oblique to one an-

The positions of the great bowlders and of the accumulations of drift correspond as to the amount of westing with the courses of the scratches.

The larger bowlders of trap and sandstone lie in the *western* half of the New Haven region. Further, they are gathered in great numbers, together with thick deposits of till, along the hilly *western border*, and especially the *eastern declivity of the high border*—as if moraine-like in origin. But some bowlders of large size occur on the top and eastern declivity of the West Rock ridge, which is one to two miles east of that western border, and a few are found farther east.

One of the largest of the trap bowlders, called the Judges' Cave, lies on the top of the West Rock ridge (at J, a mile north of Westville), at a height of about 365 feet, where it was probably stranded because 25 feet too low in the ice to clear the top of the ridge. It is now in a few large pieces, probably through the rending action of growing trees, but weighed when entire at least 1000 tons. Just a mile west of the "Judges' Cave" on the Woodbridge declivity, lies a still larger bowlder of trap and entire, its extreme length 42 feet, and estimated weight 1200 tons. The spot where it rests is about 360 feet above the sea-level, and 20 feet below the summit beyond it. But a few rods south lies another of 100 tons, and a few hundred yards to the north there is a group of five great trap bowlders, the largest about 200 tons in weight, that were similarly stranded—a most impressive assemblage. Three-fourths of a mile north *on the same declivity* lies another bowlder of 500 tons. Many more of great size—500 tons to 10 (equivalent to 6000 cubic feet to 120), occur both north and south along this border. Further, very few trap bowlders of large size passed its summit. One, measuring 13×9×5 feet, lies a mile west of it in southern Woodbridge; a group of others, the largest 17×15×5 feet, at the same distance nearly, in northern Woodbridge: and one of 8×5×7 feet in northern Orange, west of Maltby Park. Smaller trap bowlders or stones however, with others of sandstone are widely distributed for three to six miles in that direction; on the south they reach beyond Milford.*

The *course of travel* of some of these great bowlders can be made out quite closely. They were derived either from the

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other, the scratches at bottom will have an intermediate direction, which will be determined by (1) the angles of the two slopes, (2) the angle between the dip-directions, and (3) the thickness of the overlying glacier. (The velocity was too small to be included.) Hence, some variations in the direction of scratches in a given region are to be expected rather than perfect uniformity, even where there are no defined troughs or valleys.

* A trap bowlder, measuring 15×8×6 feet, lies near the Housatonic River above the railroad crossing; but it is probably from a dike that intersects the metamorphic rocks from southern Woodbridge to the mouth of the Housatonic.

West Rock Ridge or from some part of the Mount Tom range* to the north; for the more eastern trap ridges are too far east. They are, for the most part, alike in consisting of *fine-grained much-rifted* trap, the variety that occurs only as the *outside* of the ejected trap-masses where the melted rock was *rapidly cooled* against the sandstone or air. Hence they probably came from the *top* of the ridges, for the sloping eastern side is now usually under sandstone to within 100 feet (in height) of the top, and the western is that of the bold columnar front.

The trap ridges projected upward into the ice abruptly, several hundred feet—the Mt. Tom ridge (or that from Meriden to Mt. Tom in Massachusetts) probably 600 to 900 feet; a most favorable condition for the successful rending and grasping work of the glacier. The Mt. Tom ridge is 996 feet high (Guyot) in South Mountain, Meriden, 985 feet in Talcott Mountain, west of Hartford, and 1214 feet in Mt. Tom. Some of the fallen masses at the base of its bluffs near Meriden are as large as the largest of the bowlders. Moreover, the trap (doleryte) of this western trap range in Connecticut is mostly a hard, durable rock, while that of the more eastern trap range is generally a hydrous or chloritic kind, very decomposable, and having usually only small chips as its fallen masses. This is one reason why large trap bowlders are rare to the eastward.

The "Judges' Cave" certainly came from the Mt. Tom ridge, —either the main ridge or those subordinate to it, and either from the vicinity of Meriden (16 miles distant), or some point farther north. The trap beneath it is very unlike that of the bowlder. If from the Mt. Tom ridge, the direction of travel was between S. 22° W. and S. 12° W.—the latter if from the northern half of the ridge.

The 1200-ton bowlder, near the top of the Woodbridge border, was probably from nearly the same source, the rock being closely the same; and its course was therefore between S. 24° W. and S. 16° W. If the former, it had passed over the West Rock ridge, at a height of at least 500 feet; if the latter, its course was west of this ridge.

Supposing, in the case of each of these bowlders, the height whence taken to have been 900 feet, the former had sunk in the ice before landing 550 feet, and the latter 540 feet. A 500-ton bowlder that must have had the same course with the "Judges' Cave" and may have been its near companion in the ice, continued on *over the West Rock ridge* and now lies S. 21° W. of it, a mile and a half farther south (on the grounds of Mr. Donald G. Mitchell). It is about 120 feet above the sea-level—which indicates a sinking in the ice, after passing the

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* The Mount Tom range of trap extends north from Meriden to Mt. Tom in Massachusetts. See map, Plate v, in volume xxiv, of this Journal, June, 1883.

West Rock ridge, of nearly 300 feet, or half the whole amount it experienced before it landed. Two other large bowlders lie near it. It is possible that these, and others of the bowlders, reached terra firma at the final melting, or through crevasses, in which case part of the sinking may have taken place abruptly.

The distribution of bowlders and till along the western border has been stated above to be somewhat moraine-like. But it is largely a consequence of the fact that these hill-slopes, 100 to 400 feet in height above the plain fronting them, faced obliquely the advancing ice, the trend of the border being north and south, and the flow of the ice S. 12°–35° W. Consequently, as above illustrated, the stones and earth that were not high enough in the glacier to pass the summit, were dragged out or dropped. There was also diminished movement and therefore long detention of the ice-conditions against the declivity, and this favored deposition.

c. Probable slope of the upper surface of the ice down the valley.--To understand the conditions at the south end of the great valley it is necessary here to consider the question as to the slope of the ice in the direction of the Connecticut valley on which the southward movement was more or less dependent.

Wells River (a little north of the latitude of the White Mountains), is distant from New Haven about 200 miles, and from the south shore of Long Island, the supposed southern limit of the glacier, 240 miles. The height of the ice-surface about the White Mountains above the sea-level, according to the best observations, including those by Professor C. H. Hitchcock, was at least 6,000 feet and probably 6,500 feet. Assuming the height to have been 6,000 feet and this level to have extended west-by-north (the probable direction of contour lines on the glacier) over the Wells River region, the slope of the ice along the 240 miles would have been about 0·17′, equal to 4¾ feet in 1,000, or 25 feet per mile. With the height 6,500 the slope would have been 5 feet in 1,000. Either angle of slope is very small. In the Alps the lowest mean-slopes are 2½° to 3°, or 4½ to 5¼ feet per 100, a rate ten times greater than the above; but the thickness of the ice there is not over 500 feet. In Greenland, where the conditions were much like those of glaciated North America, the slope observed by Jensen over the Fredeickshaab glacier* was 0·49′, or about 75 feet per mile; and that measured by Helland on the glacier in the vicinity of Jakobshavn, 0·26′, or about 45 feet per mile. 45 feet to the mile would make the height of the ice-surface at Wells River over 10,000 feet.†

From such facts it would appear to be a safe conclusion that height of the ice-surface above the sea-level at Wells River l the White Mountains was *at least* 6,000 feet. On this umption, and supposing the slope of a line on the surface in direction of the valley equable, and the height at the south- termination on Long Island, 100 feet, the height over the ion of Windsor and Mt. Ascutney, in Vermont, would have n about 4,500 feet, and over the New Haven region very rly 1,000 feet.

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* Meddelelser om Grönland 1879, and this Journal, III, xxiii, 363, 1882.

† Even smaller angles of slope are stated to be sufficient to cause motion in

t is still a question whether the height of the ice-surface to northward was due solely to the accumulation of ice and partly to an increase in the southward slope of the land. t, since even an increase in elevation of 500 feet would make le difference in the result, it follows, if the above conclusion to the amount of slope required for movement in the valley dmitted, that

1) For *movement down the valley the ice should have had nearly uite its maximum thickness;* that is, the maximum thickness he great glacier over that region. Further, that

2) *Its flow could not have continued after melting had far ad- ced;* and further, that

3) *Movement in the valley over lower New England by local iers after the disappearance of the general glacier is improbable;* supposed local glaciers (as the scratches likewise show) were ged in the one great glacier, *and acted locally only through its* Supposing the great glacier to have melted away so as to e reduced the ice over the valley to just the limits of the necticut valley, which is nowhere over 1,500 feet deep and erally less than 1,000, it is not at all probable that with this e (the mean for the valley 3·3 feet a mile, this Journal, ii, 190, 1882) the ice could have overcome the various resist- es to motion.

The fact of a southeastward movement in the upper ice of the Glacier.

he evidence as to a southeastward movement in the upper is afforded by *glacier scratches outside* of the valley, and by *sported bowlders within it as well as outside.*

. *Glacial scratches* over the high plateaus of western and thwestern Connecticut, 800 to 1,500 feet in elevation, have ctions between S. 12° E. and S. 45° E.

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nland, but better measurements and knowledge of conditions are needed re any confident conclusion can be drawn.

my paper of 1873 (this Journal, III, v, 108), I assume a slope of at least 10 a mile, and this gave a height for the glacier of 13,000 feet in the region of supposed icy plateau on the Canada water-shed. For a reason stated beyond, surface along the line crossing the St. Lawrence valley may have been level, the slope confined to the ice above and below it; and that above may have only just enough to keep the St. Lawrence valley full to the required level.

According to my observations in Connecticut, the direction in eastern Huntington is S. 14°–25° E., and near Lane's Mine in Monroe, S. 12°–13° E., places within 12 miles of the New Haven region; in the towns of Norfolk, Preston and Kent, farther west, S. 18°–20° E.; of Warren, S. 30° E.; of Newtown, S. 32° E., and 2 miles farther west, S. 41° E.; of Sharon, S. 33°–36° E.; of Cornwall, S. 33°–36° E.; and on Mt. Washington and its highest summit, Mt. Everett, in southwestern Massachusetts (the latter, 2,634 feet high), S. 18°–29° E. The direction in Goshen, according to Mr. H. Norton is S. 23° E. and S. 38° E.; in Norfolk, according to Mather, S. 20°–25° E.; and on Mt. Tom, in southwestern Litchfield, according to E. Hitchcock, S. 17°–22° E. Other facts of similar import from eastern New York, Massachusetts, Vermont and New Hampshire, are given in my former paper. The following White Mountain observations, from the New Hampshire Report by Professor C. H. Hitchcock (vol. iii, pp. 177–195), are of later publication.

Mt. Adams, W. side, height 5,500 feet	S. 58° E.
Between Adams and Jefferson, near gap (4,939 ft.)	S. 33° E.
Top of Mt. Washington (6,293 ft.), smoothing and faint markings (at 5,800–6,000 feet, transported stones, p. 204), but striæ obscure	S. 43° E.
200 feet above Lake of the Clouds	S. 30° E.
Lake of the Clouds (5,000 ft.), the intersecting directions	S. 22° E. S. 52° E.
Between Mt. Pleasant and Mt. Franklin (4,400 ft.)	S. 30° E.
Between Mt. Pleasant and Mt. Clinton (4,050 ft.)	S. 30° E.
Near top of Mt. Clinton (4,320 ft.), N. side	S. 47°–52° E.
Mt. Clinton, S. peak	S. 50° E.
South end of Mt. Webster	S. 37° E.
Top of Mt. Webster (4,000 ft.)	S. 30° E.
Top of Mt. Moosilauke (4,811 ft.)	S. 22° E.

The abrasion observations thus far made appear to show that the *mean direction of glacier flow* in western Connecticut and Massachusetts was somewhere between S. 12° E. and S. 38° E., and probably between S. 12° E. and S. 25° E.

b. The evidence from bowlders outside of the Connecticut valley is of the same import.

Percival states that transportation over Litchfield was S.S.E., as indicated by the distribution of limestone blocks from Canaan—which means about S. 20°–25° E. A few of these Canaan limestone bowlders I have found lying now in Orange, within four miles of the New Haven region, indicating transportation for 48 to 50 miles in the direction S. 16° E. They are easily recognized by the tremolite and canaanite (massive white pyroxene) they contain. Bowlders of quartzyte, from the same region, or from Berkshire, just north, are widely distributed. Bowlders of porphyritic gneiss from the northwest are very numerous over Orange and Woodbridge.* It is not necessary to multiply facts of this kind; for it is a matter of common observation over New England that the chief part of the drift came from the northwestward.

c. *Bowlders within the area of the wide Connecticut valley prove that the southeastward movement or ice-stream extended over the valley.* Those of the unmistakable porphyritic gneiss of the Naugatuck valley, which are scattered so widely over Woodbridge and Orange, occur also in the New Haven region; and one, now three miles east of the western limit of the region is over 20 cubic feet in size. Masses of ordinary gneiss, from the same general source, are numerous for two miles east of the western border, and among them are some of 100 cubic feet and larger; and smaller ones down to a foot in diameter are of occasional occurrence for a distance of at least twelve miles eastward. Quartzyte bowlders, derived, in all probability, rom the Canaan or Berkshire region, are more numerous than those of gneiss, evidently because of their greater durability; they are scattered widely, and I have found some, probably from the same source, 15 miles northeast of the city of New Haven.

Similar facts have been observed through other parts of the Connecticut valley. Professor E. Hitchcock states, in his Massachusetts Report (1841, p. 381), that the bowlders of syenyte and granite from the west side of the Connecticut in Massachusetts are found in great numbers on the east slope of the Mt. Tom range, all the way from Mt. Tom to Hartford. To this I add that I have found a syenyte bowlder from the same source 10 miles south of Hartford, and 15 from the western limit of the old Connecticut valley. He says also (p. 605), that bowlders of quartz containing manganese and iron ores in peculiar concretionary forms, derived from Conway *west* of the valley, are abundant about Amherst, southeast of Conway near the *east* border of the valley, and Professor B. K. Emerson, of Amherst, informs me that they occur farther east in Pelham.

Up the valley to Windsor, Vermont, the same courses have been reported by Professor C. H. Hitchcock, who says, in his New Hampshire Report (iii, 263, 1878), that bowlders from Mt. Ascutney (which stands on the west border of the valley and has a height of 3,200 feet), are distributed southward along the valley to Bernardston, over 60 miles, *but also southeastward* across the valley; one, 20 feet long, 27 miles, was carried

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* The crystals of feldspar in this porphyritic gneiss are generally 2 inches long and ½ inch wide, making the rock easy to identify. It forms part of a band of gneiss to the westward of, and 10 to 20 miles from, New Haven, ranging in a N.N.W. direction from Bridgeport to Prospect, places bearing S. 45° W. and N. 12° W. from New Haven.

S. 10° E., into Surry; another 5 feet long, 35 miles, S. 8° E into Keene; others, 42 miles, S. 20° E., to the west base of Monadnock; along with which large bowlders, occur many others of smaller size.

It is thus evident that a glacial movement in a direction crossing the valley south-southeastward is as well established as that in the direction of the valley. North of Windsor evidence of both movements occurs, as already stated; southeastward scratches are most common.

3. *Correlations of the two movements as to time and drift-depositions.*

a. The valley movement continued until the general glacier-flow in the region ceased. For the scratches in the direction of the valley are the last that were then made. They are not only scratches, but over the sandstone often deep ploughings; not of occasional occurrence, but universal, every fresh removal of soil from the rock bringing them to light, and showing in Connecticut, as far as observed, no marks of any later transverse movement.

b. The valley movement was cotemporaneous throughout with the general glacier-flow. This follows from the statement on page 349 as to the thickness of the ice and the angle of slope required for a valley movement. It is indeed a necessary consequence of the fact that thickening the ice over a valley to maximum thickness would increasingly facilitate the flow in its direction, notwithstanding any transverse motion in the general ice-mass; and a thinning that would finally leave it a local glacier would enfeeble its motion or stop it altogether.

Consequently valley-movements, with exceptions in a mountain region, were not those characterizing the beginning or end of the Glacial era, but the movements that prevailed through its height. The two went on together, an *upper* general flow over a *lower* valley flow.

c. But *it may be questioned whether the upper flow kept its course unchanged quite across the great valley of the Connecticut in the States of Massachusetts and Connecticut.*

A small narrow valley, especially if steep or gorge-like, would have no valley-flow of ice, owing to friction; and the glacier would readily pass it, impeded only, if at all, by the resistance from shearing. But where the valley was larger, and had its valley flow, the case is different; yet facts that have been collected prove that this resistance was overcome in multitudes of cases over the country. And it was so in the case of the Connecticut valley in Vermont and Massachusetts, the facts above cited as to transported bowlders about the region of Amherst and Massachusetts being evidence. About

Ascutney there appears to have been some southward deflection of the *upper* or *southeastward* stream by the valley flow, for the direction of transport across is in part only 8° east of south; yet the differences of direction observed (p. 351) may have been due to the height on Ascutney from which the bowlders were taken, whether within the range of the lower current, or near the limit of the two, or far above it. About Amherst the evidence from bowlders proves clearly that the upper ice-stream continued quite across the valley. In central and southern Connecticut, the valley has a breadth of 25 miles; and positive proof that the upper current continued across has not yet been found. The distribution of bowlders indicate a flow half way across, and probably farther; but whether the upper ice did not finally in the eastern half lose its own motion and take that of the lower for the remainder of the breadth of the lower stream is yet to be ascertained. Such an event could not have happened unless the valley-flow succeeded, by its rate of discharge, in taking the southeastward slope of the overlying ice out of it, so that the only surface slope along this portion was in the direction of the valley; and were this done, the valley movement would have necessarily become the only one. Even if this were a fact, the slope of the ice-surface *east* of the eastern border of the Connecticut trough would have been southeastward, and so also the course of movement.

d. *Great deflections in the courses of the transported bowlders within the valley took place on account of their sinking from the upper or general current into the lower or valley stream.* In this way, as happens in the ocean, the lower current became the transporter and distributer of material derived from the upper. It was supplied with stones from the *north*westward and carried them *south*westward.

This fact is abundantly and strikingly illustrated in the distribution of the bowlders of gneiss. In the New Haven region they are far the most numerous, and of largest size, along the *eastern* slope of the West Rock ridge. The map shows that this ridge curves eastward at its southern extremity, making thereby a large deep corral of sickle-like outline facing almost directly the valley ice-movement, whose direction there was S. 16°–26° W. These bowlders of gneiss are most abundant over this portion of the declivity, south of Wintergreen Lake. A few hundred yards south of the lake, one of the bowlders, now in three pieces, has a length of 12 feet.

These bowlders, derived from the high land of northwestern Connecticut, must, beyond question, have passed the summit of the West Rock ridge—where between 400 and 650 feet in height—while in the upper stream of the glacier; for in no other way could they have got to the *east* side of the ridge;

and it is also beyond question that they finally got into the valley current, and were drifted back into the well-arranged corral. Some are now on the summit of the ridge; one of several cubic feet is near the "Judges' Cave;" but the larger part are a third way down the eastern slope and below this level. The gneiss bowlders are mingled with those of trap and sandstone; and nothing shows difference in time of deposition.

This kind of evidence is repeated again along the Mt. Tom ridge. Professor E. Hitchcock states in his Massachusetts Geological Report (p. 381) that bowlders of syenyte and granite, taken from the syenyte band west of the Connecticut in that State, are found in great numbers on the *east* slope of the Mt. Tom range *all the way from Mt. Tom to Hartford* Eight miles farther south, in the village of New Britain (as made known by Mr. James Shepard of that place), a bowlder of zoisite and radiated hornblende (chiefly the former), measuring about 2×2×3 feet, was turned out in grading, which must have come from a locality *west* of the Connecticut valley, in Goshen, Williamsburg or Conway, Mass., Professor Emerson informing me that this region affords just such a zoisite rock, and especially "the first row of high hills as you go up from the Connecticut valley." (Letter of June 16, 1883.) Still farther south (20 miles S. of Hartford) and just *east of a more eastern* trap ridge (6 miles farther east), extending from south of Hartford to Saltonstall Lake, I found, near the Air-Line Railroad, a syenyte bowlder 10 cubic feet in contents, which came from the syenyte band of Hatfield and Whately, Mass. (Emerson), west of the valley;* and near it lies another twice larger, of similar general aspect, but not so certainly identifiable.

Such facts prove that the phenomenon, although so remarkable, was general along the valley. They are instructive also with reference to the distance eastward to which the upper ice-stream extended over the southern part of the Connecticut valley region, though leaving the limit doubtful. They are good evidence that the two currents were moving at the same time.

4. *The direction over Long Island Sound.*

For twenty miles to the eastward and westward of New Haven bay the Sound has a mean width of 16 miles. Yet the greatest depth opposite New Haven bay is at present only 140 feet. Adding the height of the adjoining hills on its sides, the southern side of the trough has a height of 300 to 400 feet and the northern, of more than twice this amount.

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* I submitted a fragment of the bowlder to Professor Emerson, of Amherst, and he writes that it is from the "syenite" band of Hatfield and Whately (Hitchcock's geological map), it agreeing precisely with it, containing much triclinic feldspar and small square crystals of orthite."

Since the Connecticut valley trough is not continued over the bottom of Long Island Sound, or through Long Island, the valley ice, as it moved out over the area of the Sound, left behind it those confining limits which had determined its southward course; and it is a question of interest whether it found new confining limits, or not, in the sides of the Sound trough.

The trend of the Sound is N. 75° E. Now this trend is transverse to the course of the *upper* or southeastward ice-stream of the glacier, the direction of this current having been, as above shown, S. 15°–25° E.

The mean direction was between these limits quite to the borders of the Sound; for on the Sound, 8 miles east of New Haven, near Stoney Creek, glacial scratches, covering large surfaces of gneiss and granite, have the direction S. 20° to 24° E.; others, 10 to 12 miles west of New Haven, in Stratford (near the N. Y. & N. H. railroad), S. 21° to 32° E.; and 18 miles west, near Bridgeport, S. 13° to 17° E.

Further, the bowlders of Long Island show that this was approximately the direction of flow over the Sound. Trap and sandstone bowlders were carried in great quantities from Connecticut to the island, and the most abundant deposits are situated on the parts lying S. 10°–20° E. from the New Haven region where alone the Triassic borders the Sound. On the north shore of the island, between Baiting Hollow and Northville, a region bearing S. 10° E. from New Haven, the bowlders of trap are very numerous; and the sandstone fragments at one place on the shore hills are in so great quantities that they seemed at first to indicate the existence near by of an outcrop of the Triassic.

Mather mentions the same fact in his New York Geological Report, remarking, on page 170, that from Roanoke Point for three or four miles east (points between Baiting Hollow and Northville) "a large proportion of the bowlders and pebbles are of red sandstone and trap rocks, like those of New Haven and that vicinity. In some places these rocks form one-third of the mass of bowlders, blocks and pebbles." He also states (p. 171) that a mile west of Wading River (4 miles west of Baiting Hollow) "a block of fine-grained limestone containing serpentine was found; it was precisely similar to the New Haven verd antique marble." It was probably from the Milford verd antique quarry, six miles southwest of the New Haven quarry, where the rock is more largely limestone than at the latter; in which case the mean direction of travel was S. 25° E.; if from the New Haven quarry, it was S. 13° E.

On one of the hills in the interior of the island, southwest of Riverhead, near Osborn or Bald Hill, bearing S. 15° E. from

New Haven, lie several large bowlders of trap, and one of red sandstone, along with others of gneiss. One of the trap bowlders contains over 200 cubic feet. Farther east, trap and sandstone bowlders are in rapidly decreasing numbers; and the same is true of the region to the westward.*

If then, as we have shown, the flow of the mass of the great glacier was at right angles to the Sound valley, it would have tended to move the ice of the Sound against and over Long Island, and not at all in the direction of the Sound, N. 75° E. To have had an eastward movement in the direction of the Sound the surface of the Sound-ice should have had a rising slope westward, and this is opposed by the facts already stated.† It is certain, therefore, that the view that the Sound valley was excavated by the glacier is not tenable. Further, the *lower* ice-stream, or that of the Connecticut valley, struck the ice of the Sound in the direction S. 13°-25° W., which would have helped a westward flow rather than an eastward.

All the facts thus point to the conclusion that the great glacier passed, with essentialy unchanged course as far as the upper ice is concerned, from Connecticut across the Sound and the island beyond. At the same time, the existence, though sparingly, of small masses of trap on Long Island for a few miles west of the meridian of New Haven, appears to show that this valley-stream moved south-southwestward not only over the towns next west of the New Haven region but at times also nearly across the Sound before losing wholly its own direction.

5. *Effects probably consequent on the change of course in the lower, or valley, ice-current manifested in the drift depositions.*

The following are the principal facts as to the deposition of the till:

(1) The till of the New Haven region is most abundant along the eastern declivity of the western border, east of the

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* Trap bowlders occur sparingly on the eastern part of Long Island, and a few reached even Block Island. Sandstone fragments have the same range but are far fewer, because less durable. Block Island is nearly east of New Haven, but S. 20° E. from the Triassic of central Massachusetts. To the westward, some bowlders of trap are found about Port Jefferson, and rarely sandstone fragments. One chloritic bowlder, probably from two to ten miles west of New Haven, lying on the shore, measured 25 x 20 x 15 feet. Port Jefferson is south of southeastern Milford (Connecticut), and as Milford and other towns north of it, on the western border of the Triassic region, received numerous bowlders of the kind by the lower glacier current, the transportation to points so far west on Long Island is not surprising. (*Western* Long Island received its trap and sandstone bowlders, as was long since announced, from New Jersey and southern New York.)

† The movement of the great glacier, over southern Connecticut, in a direction *at right angles* to the Sound-ice implies that there was an equable slope in the surface of the glacier toward the Sound, and hence that the ice-surface over the Sound was nearly uniform from west to east, although the eastern end is half a degree farther north than the western.

Maltby Park valley (the central north-and-south depression of this border), where the depth is generally from 10 to 20 feet.

(2) The Maltby Park valley has, in general, little till over it, the surface being largely one of bare ledges and intervening marshy areas.

(3) But at one place in the eastern half of this valley (*Rd*, on the map, p. 342) *the till has a depth of more than* 107 *feet; and this piling up of till made there the isolated Round Hill* (as it is called), and gave it a height of 304 feet above mean tide, while the hills of the western border just east and southeast are only 140 to 200 feet high. Moreover these hills, and the surface along the road south of Round Hill, have not enough till to conceal the jagged rocks.

(4) The broadly rounded hills of southern Orange within three miles of the Sound, 200 to 280 feet in height, are deeply covered with till—the depth in some parts 40 feet or more.* Farther from the Sound, the till is usually of less depth, and rock-exposures are not uncommon.

The unusual amount of till and bowlders against the eastern declivity of the western border has already received explanation (p. 348).

The existence of bare ledges and little till over the Maltby Park valley is evidently a consequence of denudation; the region was swept by deep waters from the melting glacier—part going southward, directly to the Sound, with a pitch of 30 feet a mile for the four miles, and the rest northward into the valley of West River north of Westville.

The third feature mentioned is of less obvious explanation. Round Hill stands prominently in view from all directions. This isolated feature, its form, and the height of the surrounding region are shown on the following map. The great depth of the till at the summit had seemed probable from the fact that, unlike the other hills, it has no outcropping rocks over the upper third of its height; but it was *proved* by an excavation for a well made under the orders of Mr. R. M. Burwell, the owner of the place. The well was carried down 107 feet through the firmest of stoney and somewhat clayey till before water was obtained, and even at that point rock had not been reached. The highest outcrop of rock on the sides of Round Hill is at 174 feet above mean tide.

Another remarkable feature of the region is a deep trench on the southeast and east sides of the hill, only 118 to 130 feet above sea-level, something like the trench around a fortified acropolis.

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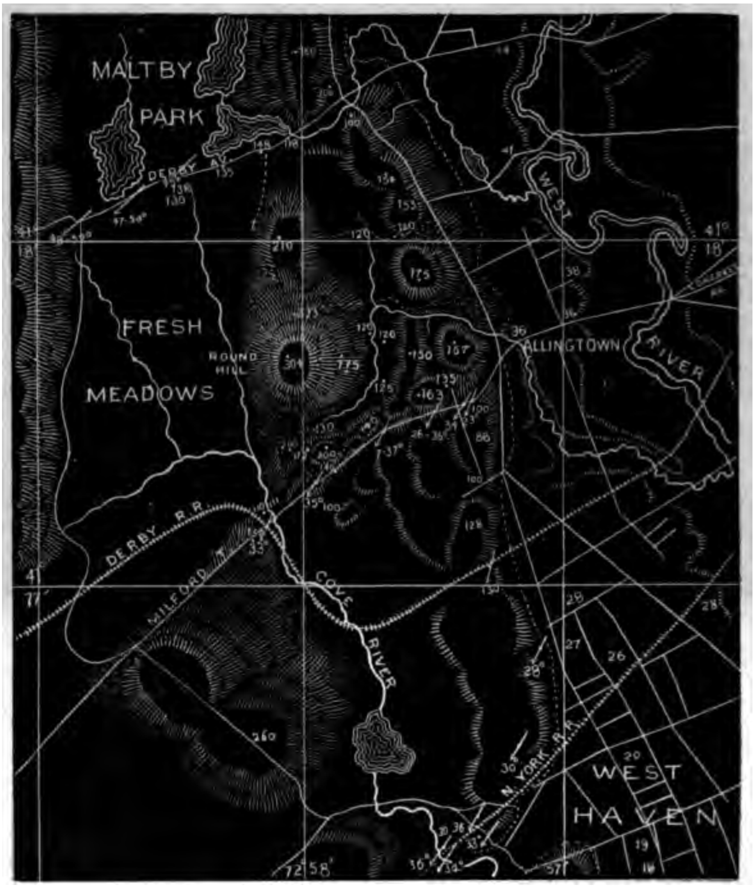
* On Shingle Hill, a little to the southwest of the region included in the map, an excavation for a well passed through 40 feet of till; on other hills, several wells go down 20 to 35 feet.

Round Hill is elongated northward—N. 8° W.; and half a mile off in that direction the height is 210 feet, and the depth



Map of Round Hill and vicinity, on the western border of the New Haven region.

of till is at least 21 feet, as found in a well-digging. It is also lengthened in the direction S. 10° W.; and only here is the slope an even one—a fact represented in the map above.* The hill thus covers and fills the eastern half of this part of the Maltby Park valley. At its western foot flows Cove River, the stream of the valley. Across this stream, another rise immediately begins, that of one of the deeply till-covered hills of southern Orange; so that the Maltby Park valley is here filled also over its western half, and consequently its breadth in this part is reduced to about 40 yards—although three-fourths of a mile wide just north. It is evident from the features of the region, moreover, that the depositions of till to the westward are essentially a continuation of those of Round Hill; that the valley was here to a considerable extent obstructed by the depositions; and that part of the flood waters from the melting glacier, besides making a lake above, flowed westward and joined the river-course next west, that of Indian River, the extensive flat meadows of Cove River north of the obstruction being elongated in that direction.

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* It has the constitution, as the description shows, of the "lenticular hills" of Professor Edward Hitchcock. But the quality *lenticular* is not an essential feature of such till-made hills, though common, as it is, and for a like reason, of many of the rocky ledges of the same region.

With reference to the origin of Round Hill, we note that the direction in which it is elongated is most nearly that of the *lower*, or valley, ice-current; and if we regard the more southwestern depositions as its continuation, the direction becomes S. 10°–15° west, which is yet nearer the course of that current, though still differing by 15 to 20 degrees. The actual direction of the ice-movement around it is registered at many places in glacial scratches. The courses are indicated on the map by arrows. They average S. 33° W. to the south and southeast of the hill, while to the north, the directions S. 45°–56° W. occur.

It is plain, from these facts, that the making of the solitary hill cannot be accounted for on the supposition of an eddy in the flow of the ice. The uniformity in the course of the scratches shows that there was no eddying.

The position upon the western border, where there were large drift depositions, and must have been, as already suggested (p. 348), the frequent opening of crevasses, may have had something to do with its formation. But no *line* of crevasses would have made so local a deposit.

Another point with regard to its position is its nearness to the Sound—the distance about three miles; and in this fact and the preceding, in connection with the character of the ice-movement, we have apparently the only data for an explanation. Over the Sound and its borders, the lower or valley ice-stream became warped around into the course of the general glacier-mass. This forced bending must have produced its greatest crevassing effects over the southwestern part of the valley region, within the plane of contact between the upper glacier stream and the lower—a plane that rose from the surface of the Sound at a small angle; and these effects would have been at a maximum in the part of the glacier where this plane intersected the north-and-south crevassed course of the western border. The existence is therefore probable of a knot of profound crevasses on this border region, at some point of greatest pressure, not far from the Sound; and, if a fact, a stream of water, or river, from the upper surface of the glacier may have here plunged down—like some rivers in glacial Greenland—causing a local deposition of the stones and earth that were in the ice, and thus have located and formed the till-made hill.

If the change of course were due only to the slope of the upper surface of the ice *at the place* it might have gone forward quietly. But if partly due to pressure from the northward along the valley there would have been the resistance and wrenching supposed.

The plunging waters would account also for the size and position of the half-encircling valley, cut in the schists about Round Hill; for this valley's being mostly free from drift; and for the trench which is its continuation eastward to the New Haven plain; for this was the way of discharge of the descending waters.

But the stream of water descending a crevasse could deposit only the drift encountered in its course on and through the ice; and wherefore then so high a pile in Round Hill? And why were 19-20ths of the drift in the hill the contributions of the valley or lower glacier current?

The height is evidence of long-continued deposition.

The valley source of the material proves that the drift of the glacier was mostly confined to the lower ice.

The mixture along with the trap and sandstone of some porphyritic gneiss, quartzyte, etc., from the northwestward, and the increased amount of the same in the western part of the hill, shows that the upper ice-stream was supplying material at the same time with the lower, though in much smaller amount.

The height of the hill, in connection with the nature of the material, indicates, further, that the lower, or valley, ice-stream had a depth in that region of at least 300 feet.

Round Hill is an example of a "kame." It is probable that the Andover kames were located by *crevasses* in the glacier that overlaid the region when the material was deposited.

Other facts bearing possibly on the origin of Round Hill will be presented in the following part of this paper—On the Terrace (or Champlain) formation of the New Haven region, its deserted valleys and its "kettle-holes."

Conclusions deduced in the preceding pages.

1. Two movements existed in the glacier-ice—a lower along the valley, an upper crossing it obliquely.
2. The two movements were simultaneous.
3. The upper ice kept its motion over the southward-flowing valley-ice quite across the Connecticut valley in Massachusetts, and across ten miles at least of its breadth in the southern half of Connecticut.

4. The lower or valley ice-stream was most rapid when the general glacier was of maximum thickness.

5. The glacial flow probably ceased when the melting was half completed, except in the mountain regions and some special localities elsewhere of limited extent; in which case, the disappearance of the general glacier did not end in leaving one or more long local glaciers in the lower half of the Connecticut valley.

6. Both the upper and lower ice-streams transported drift-material. The material from the upper that sunk into the lower stream was carried by it south-southwestward and deposited with the drift of the latter, and often against the eastward slopes of obstructing ridges.

7. The lower ice-stream lost its own direction of flow on reaching Long Island Sound, and took that of the general glacier mass across the Sound and Long Island.

8. The obstruction to flow in the western border of the New Haven region was the cause of crevasses and much drift deposition; and between this action and the warping around of the valley ice near and over the Sound, causing intersecting crevasses, the high local till-deposit of Round Hill was produced.

ART. XL.—*On a variety of Descloizite from Mexico;* by SAMUEL L. PENFIELD.

DURING the spring of 1883 Professor Wolcott Gibbs of Cambridge sent some specimens of a vanadium mineral to Professor George J. Brush for identification. The specimens seemed to be fragments of a coating from one-eighth to three-eighths of an inch in thickness, composed of groups of prismatic crystals arranged somewhat like the common crystallizations of prehnite or calamine. On the top surface of one specimen minute crystal faces were visible, very irregularly grouped together and curved, but in the other cases it simply appeared rough and somewhat weathered. The structure on the cross fracture was columnar-fibrous, sometimes radiated; it showed at all times a fresh and homogeneous interior of a dark brown color and resinous luster very much resembling some varieties of sphalerite. Cleavage distinct, parallel to the columnar structure. Hardness 3·5; specific gravity 6·200–6·205.

Before the blowpipe it gave reactions for vanadium, arsenic, lead, copper and zinc. Heated in the closed tube it fused very readily, boiled up violently, and gave off water which reacted neutral to test paper.

The specimens were said to be from near Zacatecas, Mexico, and were sent to Dr. Gibbs by Herbert G. Torrey, Esq., of the U. S. Assay office in New York. In all physical properties the mineral seemed identical with *tritochorite* recently described by A. Frenzel* from an unknown locality in either Mexico or South America. Whether identical with Frenzel's mineral or not it was regarded as worthy of further investigation and Professor Gibbs kindly provided me with the material for analysis.

Various methods of analysis were tried but the following was found to be the most satisfactory. The mineral was readily soluble in warm dilute nitric acid; to this solution enough sulphuric acid was added to combine with the bases and the whole was evaporated till nitric acid was completely expelled; after cooling, cold water was added and after standing for some time the lead sulphate was filtered off and weighed with the usual precautions. The lead sulphate was soluble in ammonium acetate, except a slight residue of silica and a trace of vanadium, both of which were taken into account. No trace of vanadium went into solution in the ammonium acetate, so that the separation was almost complete. To the filtrate from the lead sulphate hydrochloric acid and sulphurous acid water were added and allowed to stand. The sulphurous acid was subsequently expelled by boiling and copper and arsenic precipitated by means of hydrogen sulphide, filtered off and washed. After drying the sulphides of copper and arsenic they were transferred to a dry beaker and dissolved in nitric acid. The filter paper was treated in another glass with nitric acid and potassium chlorate to destroy organic matter. The solutions were then evaporated to dryness on a water bath, the residue dissolved in water and the arsenic acid precipitated by means of magnesia mixture as ammonium magnesium arsenate. This was collected on an asbestos filter and weighed first after drying at 110° C., and then again after ignition, as pyrophosphate. The copper was precipitated from the filtrate from the ammonium magnesium arsenate by means of hydrogen sulphide and weighed, after ignition in hydrogen, as subsulphide. The filtrate from the first arsenic and copper precipitate was evaporated to dryness on the water bath and finally ignited to expel as much of the sulphuric acid as possible. The residue was moistened with strong nitric acid to oxidize the vanadium and evaporated to dryness on the water bath. The residue treated with water and ammonia gave a solution in which there was a flocculent yellow precipitate of zinc vanadate; by adding acetic acid to acid reaction this precipitate dissolved, leaving only a slight precipitate of vanadate and phosphate of iron; this latter was filtered off, washed, ignited, moistened with nitric acid and again ignited and weighed. It was then fused with sodium carbonate, the fusion soaked out in water and the oxide of iron thus separated and weighed. The soluble part of the fusion contained both vanadic and phosphoric acids. To the dilute acetic acid solution containing the vanadium and zinc, lead acetate was added which precipitated all the vanadium as a basic lead vanadate. This precipitate was washed with hot water and dried; it was then transferred to a beaker, the paper burned and the ash added; the whole was then dissolved in nitric acid, sulphuric acid added to precipitate the lead and evaporated till nitric acid was expelled. The vanadic acid was then dissolved in water, filtered from the insoluble lead sulphate, evaporated in a weighed platinum dish and after expelling the sulphuric acid and strong ignition, weighed as pentoxide. As regards the purity of the vanadium pentoxide obtained by the above method the following can be said. It was completely soluble in aqueous ammonia and the solution when transferred to a weighed platinum crucible and evaporated and ignited gave results agreeing with the weight obtained by weighing in an open platinum dish. The pentoxide was reduced to the trioxide by igniting in a current of hydrogen gas and gave in two cases a loss of oxygen equal to 16·86 and 16·98 per cent of the pentoxide employed. Vanadium pentoxide with molecular weight 192·4 should give by reduction to the trioxide a loss of oxygen equivalent to 17·54 per cent.

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* Tschermak's Min. und Petr. Mittheilungen, iii, 506; iv, 97.

The zinc which remained in the filtrate from the basic lead vanadate was precipitated, along with the excess of lead used as a precipitant, from the acetic acid solution by means of hydrogen sulphide. The sulphides after settling were collected on a filter, washed and dried; they were then transferred to a dry beaker, the filter paper burned and the ash added; to this strong nitric acid was added with enough sulphuric acid to combine with the bases, and the whole evaporated till sulphuric acid fumes began to be given off. After cooling and addition of water the soluble zinc sulphate was separated from the lead sulphate by filtration. The zinc was then precipitated as basic zinc carbonate by means of sodium carbonate with the usual precautions, washed and weighed as oxide. The zinc oxide was in all cases tested and found to be free from vanadium.

Phosphoric acid was determined in a separate portion by means of ammonium molybdate. The first precipitate seemed to be impure; it was therefore dissolved and reprecipitated with ammonium molybdate before its solution in ammonia and precipitation with magnesia mixture.

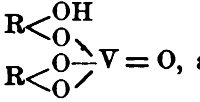
Water was obtained by igniting a weighed portion of the mineral to low redness and calculated from the loss of weight. To guard against any inaccuracy another determination, analysis III was made by igniting in a Bohemian glass tube and collecting and weighing the water in a chloride of calcium tube. The air-dry mineral did not lose weight by heating to 100° C., and lost only 0·12 per cent by heating for two hours at about 360° C.

The results of the analyses are as follows:

	I.	II.	III.	Mean.	Ratio.	
V_2O_5	19·00	18·90	...	18·95	·104	·121
As_2O_5	....	3·85	3·78	3·82	·016	
P_2O_5	·18	....	....	·18	·001	
PbO	54·99	54·91	54·89	54·93	·247	·484
CuO	6·76	6·79	6·68	6·74	·085	
ZnO	12·14	12·34	12·23	12·24	·151	
FeO	....	·06	·06	·06	·001	
H_2O	2·73	2·65	2·72	2·70	·150	
SiO_2	·12	....	....	·12		
				99·74		

The ratio of the pentoxide to the protoxide to water = ·121 : ·484 : ·150 = 1 : 4·00 : 1·24 = 4 : 16 : 5. If we assume the ratio to be 1 : 4 : 1 we obtain the formula $R_3(VO_4)_2$, $R(OH)_2$ or $R_2(OH)VO_4$ which is now regarded as the true formula for descloizite. It does not seem probable that the ratio 4 : 16 : 5 giving a complicated formula, is correct. The excess of water is, however, not due to the presence of hygroscopic water, and especial care was taken, as explained above in the description of the method, to obtain an accurate and sharp determination.

Unfortunately our knowledge of descloizite is somewhat uncertain, but it probably has the formula $R_2(OH)VO_4$ or



$$\begin{matrix}\mathrm{R}<\begin{smallmatrix}\mathrm{OH}\\ \mathrm{O}\end{smallmatrix}\searrow \\ \mathrm{R}<\begin{smallmatrix}\mathrm{O}\\ \mathrm{O}\end{smallmatrix}\nearrow\end{matrix}\mathrm{V}=\mathrm{O}$$

, and with our present knowledge I prefer to refer the vanadate, which has been analyzed, to this species, it differing only in having a part of the vanadic acid replaced by arsenic acid.

As to the purity of the material which was analyzed, I will state that the specimens were apparently homogeneous, and two thin sections which were prepared revealed no impurity when examined with the microscope.

In regard to the relation between this mineral and Frenzel's tritochorite, little can be said. He states that his mineral, although from an unknown locality, is from either Mexieo or South America, whence it was brought to England or France for the manufacture of vanadium compounds. His description of the mode of occurrence and physical properties is almost identical with the specimen in my possession. The minerals have the same density and are alike in many particulars as seen by the following comparison of the analyses.

		V_2O_5	As_2O_5	P_2O_5	PbO	CuO	ZnO	H_2O	
Tritochorite, Frenzel G.,	6·25	24·41	3·76	....	53·90	7·04	11·06	....	= 100·17
Descloizite, Penfield,	6·205	18·95	3·82	0·18	54·93	6·74	12·24	2·70	

It does not seem probable that 2·70 per cent of water could have been overlooked in the analysis of tritochorite and the difference in the percentage of vanadic acid in the two minerals is also quite large. Nevertheless, the minerals are so much alike in all physical characters, that it may be possible that they are identical. But more definite facts in relation to the occurrence of the minerals and further analyses are necessary in order to decide definitely the relation between them.

Sheffield Scientific School, June 23d, 1883.

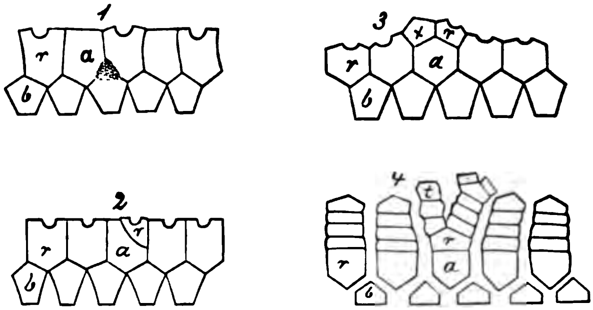
ART. XLI.—*On Hybocrinus, Hoplocrinus and Bærocrinus;* by CHARLES WACHSMUTH and FRANK SPRINGER.

WHEN we prepared the generic description of *Hybocrinus* for the first part of our Revision of the Palæocrinoidea, we were not aware that the Duke of Leuchtenberg had described a species from Russia under the name of *Apiocrinus dipentas*, which had been referred by several writers to the former genus. Our first knowledge of the occurrence of this genus in Russia was derived from Professor Zittel's Handbuch der Palæontologie, received while our work was in press, in which Zittel placed *Hoplocrinus* Grewingk, *Homocrinus* Eichwald (not Hall), and *Bærocrinus* Volborth, as synonyms under *Hybocrinus* Billings. Unable at that time to obtain the necessary literature and over-anxious to have the generic records as complete as possible we copied Zittel's synonymic list, quoting him as authority, and left the recording of the species, of which we knew nothing, for some future time. That we gave simply a quotation from Zittel was overlooked by P. Herbert Carpenter in his late paper "On the Relation of *Hybocrinus, Bærocrinus* and *Hybocytites*," published in the Quart. Journ. Geol. Soc., London, August, 1882, page 292. Carpenter found it "rather curious" that we had not observed the differences existing among the azygous plates of the Russian and Canadian species, and while quoting *Hoplocrinus* and *Bærocrinus* as synonyms of *Hybocrinus*, had not noted the species described under those genera.

We have since had opportunity to examine the numerous publications which have appeared on these genera and have come to the conclusion that both Russian forms should be separated from *Hybocrinus* even more than subgenerically.

The genus *Hybocrinus*, as originally defined by Billings, is composed of five basals (no under basals), five radials and two azygous plates. Four of the radials are equal and alternating with the basals; the fifth much smaller and resting upon the right sloping side of a large azygous plate which stands in line with, and has about the size of the four large radials. The left sloping side of the azygous plate is occupied by a second azygous piece, which in form, size and position closely resembles the fifth radial lying adjacent to it on the right. There are only five arms, undivided, and apparently devoid of pinnules.

The Russian form (fig. 2), is very similar to the American. It has the same humped appearance; also five simple arms, and the plates in the calyx are similarly arranged, but contrary to the American it has only a single azygous plate. The left side of this plate extends to the upper margins of the four large radials, while its sloping right side supports the fifth radial, which is much smaller and triangular, and taken together with the azygous piece as one plate the two resemble in form the four large radials, only that there is an arm facet and this pushed to one side. The Russian form, which is represented by Leuchtenberg's *Apiocrinus dipentas*, was, curiously enough, regarded by Eichwald (Lethaea Rossica, Band I, pp. 582–584), as identical with *Homocrinus* Hall, a genus with well-developed under-basals, to which he referred the species.



1, *Bœrocrinus;* 2, *Hoplocrinus;* 3, *Hybocrinus;* 4, *Iocrinus.*
b, basals; *r*, radials: *a*, azygous plate; *x*, special anal plate; *t*, plates of the tube.

Under *Homocrinus dipentas*, Eichwald figures two specimens, the one Leuchtenberg's type from the Vaginatenkalk of Pulkowa, near St. Petersburg, the other from the Brandschiefer of Erras. The two specimens, which in their external appearance differ considerably, were afterwards referred by some authors even to the same species, while others have described them under distinct genera. The Erras specimen, which was referred by Volborth (Bulletin St. Petersb. Acad., vol. viii, 1865, p. 178), to a new genus, is known as *Bœrocri-*

nus Ungerni; that from Pulkowa was placed by the same author under *Hybocrinus* Billings. The differences which exist between the latter form and the Canadian types of that genus were not observed by Volborth, but they were pointed out by Grewingk (Archiv. für die Naturkunde, Liv. Ehst. and Kurland's Ser. 1), vol. iv, p. 110, who placed only the Canadian species under *Hybocrinus,* and proposed for the typical Russian form the genus *Hoplocrinus,* accepting *Bærocrinus* for the Erras specimen. Eichwald, in reply, vigorously defended his first opinion, maintaining that the Erras specimen represented a monstrosity, that it was specifically identical with the Pulkowa specimen, and that both belonged to *Homocrinus* (Bulletin de la Soc. Imp. des Nat. de Moscow, ii, p. 150). Volborth responded (Ibid, p. 442), insisting that a species composed of two rings of plates could not belong to a species having three; that the Pulkowa specimen was a *Hybocrinus,* and that the Erras specimen was specifically and generically distinct therefrom.

Schmidt, who wrote several years later (Memoires de l'Acad. Imp. des Sci. de St. Petersb. (Sec. 5) vol. xxi, No. 11), acknowledged the differences between the Canadian and Russian species, but he regarded them as not of sufficient importance for generic separation. He took the Erras specimen to be an abnormal, imperfectly formed example of *Hybocrinus dipentas.*

Carpenter (Quart. Journ. Geol. Soc. London, August, 1882), agreed with Schmidt and Volborth that *Apiocrinus dipentas* was a *Hybocrinus* but only the typical form. He considered the simple azygous plate in the Russian species as "probably equivalent to the two present in the Canadian species," and he proposed a modification of Billings' generic definition, so as to include the Russian form; but he considered *Bærocrinus,* as defined by Grewingk, to be "an altogether different generic type."

The differences in the number and arrangement of the plates at the azygous side have been very generally regarded as excellent characters for distinguishing genera among Palæocrinoidea, and in the Cyathocrinidæ, which show but little variation in the general construction of the body, they frequently constitute, when the arms are unknown, the only means of separation. That in the present instance the two types happen to resemble each other in their umbonate form, can be no valid reason for making these genera an exception to the general rule, since all Cyathocrinidæ are more or less protuberant along the anal regions.

It has been satisfactorily proved that there exists a structural difference in the azygous plates of the respective types;

Volborth has established a genus for the Russian form and this cannot be disregarded without great inconsistency. That the one azygous plate in the Russian species is probably equivalent to the two in the Canadian, which we do not dispute, does not in our opinion alter the case. We have already shown (Revision I, pp. 65–75), that the differences among the plates of the azygous side in some of these genera are the result of modifications from one to another, and we hope to prove further on that the plates which constitute the azygous side, both special anal plates and adjoining radial, had a common origin in all these genera, and were gradually evolved from a simple azygous plate.

There is, however, another good distinction between *Hybocrinus* and *Hoplocrinus*, to which, as yet, little attention has been paid, although it has been indicated by Wetherby (Cincin. Journ. Nat. Hist., July, 1880), when he described the upper azygous plate "as rounded and crenulated at its distal extremity." Wetherby had discovered in Mercer county, Ky., associated with *Hybocystites problematicus*, in a siliceous limestone at the upper part of the Trenton group, several well-preserved specimens of *Hybocrinus tumidus* Billings, which he illustrated by several figures. Fig. 2 on Plate V, represents the azygous side of a specimen "showing the crenulated and convex upper face of the azygous plate," a structure which had not been found in any of the Russian specimens, and which they probably did not possess, if reliance can be placed upon the figures.

Wetherby states that in *Hybocrinus* "the form of the (azygous) plate is sufficient evidence, that it supports a strong ventral sac," and he says further, that "the crenulated condition of the articulating upper surface of this plate indicates the place of the lower exterior opening into this sac."

We are sorry that we cannot agree with Wetherby in some of his conclusions. We think the form of the azygous plate, on the contrary, indicates that the species had an unusually short ventral sac, or actually no sac at all, but was provided with a bulbous protuberance, in connection with—and forming a part of—the azygous (anal) plate. The structure is not well shown in the figure, and probably none of the specimens which had been discovered up to the time he wrote, exhibited satisfactorily the crenulated portions which form the upper part of the protuberance. We have, however, been favored lately with several excellent specimens, which Professor Wetherby has since obtained from the same locality, and these have afforded us somewhat better information respecting these parts.

The second azygous plate, up to the place where the crenulation begins, is irregularly quadrangular, the upper side rounded and convex. The "crenulated part," which is much more delicate, and rarely preserved in the fossils, at first takes an upward direction, but soon bends abruptly inward, then downward toward the ventral cavity. The part comprising the latter side is not well shown in any of the specimens, but so far as we can judge, it seems that the crenulation does not extend beyond the upper edge. There is seen, however, in one of our specimens at that side a small projection, with possibly an opening, which may represent the anal aperture. The crenulated portion of the plate is continuous with the smooth part, there being no suture between them, but the division is marked by a well-defined curved ridge across the plate, which extends down a little lower next to the small radial, than at the opposite side. From this ridge pass out, nearly at right angles, numerous parallel grooves, which take an upward and inward course, and extend to the upper end of the plate. The grooves are so regular and deep, that they are evidently no mere surface ornamentation, but represent a complicated organic structure. The protuberance extends somewhat beyond the limits of the radial plates; it is compressed, being wider than deep, and its general appearance that of the rhombs in the Cystidea. Indeed the resemblance to the latter is so striking that we think the grooves of *Hybocrinus* communicated, like those of the rhombs, with the inner body.

From Wetherby's description we infer that he supposed the hollow portion of the anal piece to be solid, for he took the upper side of the crenulation to be "the articulating surface" of the plate, and heavy-plated, when in fact it is delicate and hollow within. Thus only can we explain his supposition that the azygous plate was supported by a strong ventral sac. He evidently thought the so-called "articulating surface" had been surmounted by other plates, and that these had formed the sac. In using the term "articulating surface," Wetherby probably did not think of a connection of muscles and fossæ, as supposed by Carpenter (Quart. Journ., 1882, p. 305), nor of any other movable mode of articulation; he simply meant the line of junction by suture. The word "articulation" has been frequently used in the description of Crinoids in the latter sense, and we have ourselves used it so; but it is not a good practice, and should be abandoned. A connection by suture ought to be called so, and the above term be used only when there is a movable connection or true articulation.

We suggested (Revision I, pp. 66 and 74), that in *Hybocrinus* the small radial corresponded to the upper half of the compound plate in *Dendrocrinus*, and that the lower half was probably represented in a portion of the large undivided azygous plate. We further alluded to *Homocrinus* as differing from *Dendrocrinus* only, in having the suture between the sections of the compound plate sloping instead of horizontal, and that thereby the plate which in the latter had been a radial in position, had become, by slight modifications, transformed into an anal plate. These views were accepted by Carpenter, who expresses himself as entirely concurring with us respecting the mutual relations of *Hybocrinus*, *Homocrinus* and *Dendrocrinus*, but who suggests that probably the lower part of the compound radial of the latter genus was represented by the whole of the large azygous plate of *Hybocrinus*, and not by a portion of it, and in this he is undoubtedly correct. He disagrees with us, however, in regard to the relations of these genera to *Iocrinus*.

We described *Iocrinus* (Revision I, p. 68) as being composed of five basals, and five equal radials; the latter supporting three or four brachials, of which the proximal one of the right postero-lateral ray was bifurcating, toward the right supporting four brachials, and toward the left giving off a large ventral tube. We stated that the bifurcating plate, which to us appeared to be movable like the succeeding pieces, had a strictly radial position, but that it supported not only arms, but also the ventral sac, and that this plate, resting upon what we considered to be a true radial, was a brachial with interradial functions. We admit that the large so-called postero-lateral radial (fig. 4*a*) is not articulated to the bifurcating plates but united with it by suture; we insist, however, that the latter cannot be an azygous plate, as suggested by Carpenter,—nor is it a brachial—but that it is the equivalent of the combined small radial, and small anal plate in *Hybocrinus*, and that the large plate underneath (fig. 4*a*), which both Carpenter and ourselves took to be a radial, is an azygous plate, analogous to the large azygous plate in *Hybocrinus*. The change which we propose in the generic formula of *Iocrinus* will be better understood, after an examination of *Bærocrinus*.

The original generic description of *Bærocrinus* by Volborth contains little information respecting the arrangement of the plates in the calyx, of which the greater part in the type was concealed by matrix. Volborth distinguishes *Bærocrinus* from *Hybocrinus dipentas* by the greater size of the body and slight differences in the structure of the arms. He describes also a peculiar "elliptic organ," placed along the lower corners of two adjoining radials and across the angle of the adjacent basal. This so-called "Volborth's organ" stands somewhat elevated above the calyx, where it forms a kind of excrescence upon the surface. It is said to be closed by numerous small polygonal pieces, so arranged that their sutures are nowhere continuous with the sutures from the adjoining larger plates.

this peculiar organization, mainly, which Volborth took either a madreporic body or a genital organ, the genus *crinus* was established.

1867, Grewingk brought forth an amended description of *crinus.* He had succeeded in cleaning the Erras speci- from its matrix, and this had enabled him to examine the of the body and the construction of the plates, of both of h he gave figures and diagrams. He declared Volborth's to be a monstrosity, and undertook to prove this by the ularity in the arrangement of the so-called plates of which sists. According to his version, it is composed of about een pieces, of the most irregular form and size, some of pentagonal, some hexagonal, others rounded, and all out definite arrangement. He further states that the es from the adjacent radials and the basal plate are not nued within its limits, and that nothing leads to the usion that these plates meet with each other, like the ining radials and basals of the specimen. He thinks the ing is produced by the breaking up of the plates sur- ling it, and he finds that at least some of the cracks do ass entirely through the walls. He describes the calyx as osed of five basals and five radials, the latter arranged in tinuous series, but three of them only provided with an lar facet and arm-bearing. Among his figures there is a am (fig. 2*d*) showing the outline of the body as it appears upper edge of the radial plates, which is triangular, with ht sides and rounded angles. The angles represent the uding arm facets of the three arm-bearing plates, while wo non-arm-bearing pieces form a part of the sides of the gle. Looking at this figure, and afterward at fig. 1*c*, gives the same view of "*Hybocrinus*" *dipentas*, which presented with a strictly pentagonal outline, the contrast rs to be most striking; but in his description Grewingk little attention to this feature, and only mentions it by g that the form of the calyx is "globular, with the excep- of the upper rim which is triangular." Volborth seems to considered the five large radials, of which only three are earing, and to some extent perhaps the "ellipische Aus- tte zwischen den Klammern der Armglieder," the prin- generic characters of *Bærocrinus.*

midt, who wrote in 1876, considered the Erras specimen normal and imperfect form of "*Hybocrinus*" *dipentas.* ad seen in the collection of Volborth three specimens Palowsk, which possessed only four arm-bearing radials— of the right anterior ray having no arm facet—together others in which all five radials were developed, and he the former to be an important link between the five-armed specimens and the three-armed one from Erras. As to the absence of the small posterior radial, he thinks it may have been lost in the specimen. He attributes Volborth's organ to mechanical causes and deems it to be of no value specifically or generically. According to his statement, the calyx bulges out and is torn up by rents and cracks, which follow all possible directions, forming small irregular plates, and that some of the cracks extend deeply into the surrounding plates (fig. 1 on pl. 1). Schmidt explains the "Ausschnitte" between the arm joints in the Erras specimen as incidental breaks, which he attributes to the delicacy of the plates toward the articulation. Nothing is said about a difference between the two types in the form of the body.

According to Carpenter, the last writer upon the subject, the cylyx of *Bærocrinus* "consists of ten plates, which are arranged in two alternating rows without any indications of anal plates," and he remarks—"this does not agree at all with Billings's analysis of the *Hybocrinus* calyx." He says further: "*Bærocrinus*, if rightly described by Grewingk, represents to my mind an altogether different generic type," and "occupies a somewhat unique position among the Crinoidea. It is, perhaps, best regarded as a permanent larval form, which has only developed three of its five arms." And he says of Volborth's organ—"it struck me as possible that it may represent the anal opening, which does occupy a somewhat similar position between the radials and basals at one period of Crinoid development." He also alludes to the triangular outline of the calyx in *Bærocrinus*, and the pentangular form in "*Hybocrinus*," which he thinks additional proof that the two are distinct types.

It seems that Carpenter's arguments for separating *Bærocrinus* from "*Hybocrinus*" (Hoplocrinus) are based upon the following points: The former has no anal plates; only three arm-bearing radials; possibly an anal opening between basals and radials; and the calyx has a different form. It has been stated that neither Grewingk, who gave a cross section of the upper part of the two specimens, nor Schmidt, who saw those figures and examined the original specimens, attached any importance to the form as a distinguishing character, and it seems to us that a careful comparison of Grewingk's figures 2*d* and 2*b*, which both represent the Erras specimen, disclose the fact, that the trigonal outline of figure 2*d* does not extend much beyond the very edge of the calyx; that the cross section beneath the arm facets is pentangular, or as much so as in the Pulkowa specimen, and that even the upper edge would represent a similar outline if the two non-arm-bearing plates had been provided, like the other three, with protruding facets.

We think the trigonal form in this case is fully explained by the absence of these two facets. The outline could not be otherwise, and for a similar reason it must be quadrangular in the four-armed Palowsk specimens.

The non-development of a radial, which is found occasionally among specimens of the early types, is evidently, as Carpenter admits, due to their low organization, but for the same reason this feature cannot be of much value in considering the generic or even specific relations of these forms.

Carpenter's suggestion that Volborth's organ was perhaps the anal opening seems to us to be sustained neither by embryology, nor by its own structure. When the Pentacrinoid larva of *Antedon* assumes the crinoidal type, almost contemporaneously with the development of the first radials, there appears between two of the latter plates, and on a level with them, an irregular single anal plate. Somewhat later this attains an elliptic form, and is gradually lifted out from between the first radials, until it becomes developed into a conspicuous funnel. At the end of *Pentacrinoid* life the anal plate disappears entirely, being removed by absorption over the whole body.*

We find a close analogy to the anal plate of the young *Antedon* in the simple, large anal piece of *Hexacrinus* and *Dichocrinus*, in which that plate occupies the very same position, and is surmounted also by a funnel-shaped tube; but we confess that we fail to find any such analogy in the so-called organ of Volborth. In the Erras specimen there is, in place of the one plate, a multitude of plates of the most doubtful origin, and these occupy what ordinarily would form the junction between two radials and a basal plate. We find no indication of an aperture, and none is mentioned by either Volborth or Grewingk. Schmidt's figure, pl. 1, fig. 1, which is probably the most reliable, shows a long gaping crack through the middle part, but this is evidently a break and no regular organic structure. The irregularity of all these plates seems to us also a serious objection to Carpenter's view, as the small plates surrounding the anal aperture, in all Palæocrinoids, are arranged with the utmost regularity, and in all cases where there is a lateral opening, their arrangement presents a strictly bilateral symmetry. All this leads us to think, that we cannot be far amiss, in considering Volborth's organ, as Eichwald, Grewingk and Schmidt have done, to be an abnormal structure, due to mechanical agencies. Its protruding form would indicate that this force must have been pressure from within, and hence that this structure, very probably, may represent an abnormal anus, formed when the regular anal opening of the specimen was functionally defective. We have frequently seen specimens, particularly among Actinocrinidæ, in which the anal passage had been obstructed, and a new opening had formed by breaking through the test at some other place. In one remarkable specimen of *Actinocrinus*, which has been noticed already by one of us (this Journal, vol. xiv, p. 127, Aug., 1877), the new opening penetrated the calyx along the basi-radial suture, between the first anal plate and the right posterior first radial, forming a large excrescence, composed of numerous irregular pieces; while in other specimens similar openings protrude but little above the general surface of the calyx. In every case in which this sort of anus has been observed, whether passing through the vault or the calyx, it is found to be located within the anal interradius, and hence we may safely assert that if Volborth's organ served such an office, the side which it occupies is the anal side, and that the non-arm-bearing plate toward the left is an azygous plate, and not a radial as suggested by Carpenter and the other writers. If it is a radial, *Bœrocrinus* represents an exceptional type. It was Carpenter himself, who together with R. Etheridge, Jr. (Ann. and Mag. Nat. Hist., April, 1881, p. 295), first pointed out, that in Paleozoic Crinoids the symmetry of the calyx was more or less disturbed by the presence of an azygous or anal side, and this was the most constant character for distinguishing the Palæocrinoidea and Neocrinoidea. Now, if *Bœrocrinus* is composed of five basals and five radials without an anal plate, it surely would represent one of the most symmetric forms of the Palæocrinoidea, such as are rarely found except toward the close of the Subcarboniferous.

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* Researches in the Structure, Physiology and Development of *Antedon* (*comatula* Lamark) *rosaceus*, by W. B. Carpenter, M.D., F.R.S., Philosophical Transactions of the Royal Society, London, clvi, pp. 726 to 747.

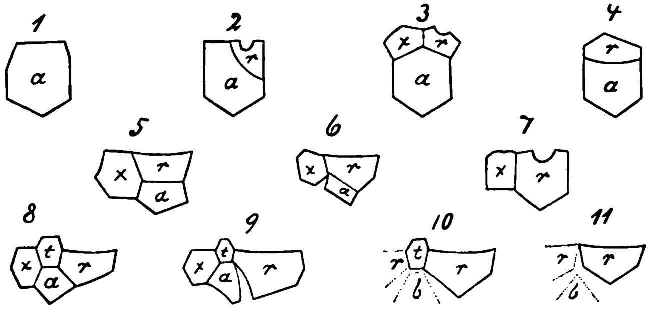
We offer the following amended diagnosis of *Bœrocrinus:*

Calyx composed of five large nearly equal basals, and a row of five other plates forming a second circlet. The plates of the latter consisting of four radials, three of them—perhaps sometimes four—arm-bearing, and a large undivided azygous plate of similar form as the radials. The plate, which in other genera represents the right posterior radial, is not developed. Arms similar to those of *Hoplocrinus* and *Hybocrinus*.

Comparing this formula with that of *Hoplocrinus*, there seems to be a wide difference between the two types. But looking at the diagrams, figures 1 and 2, and remembering that in *Hoplocrinus* one of the large radials, as shown by Schmidt, is occasionally not arm-bearing, we find that a single suture drawn across the right upper corner of the undivided azygous plate of *Bœrocrinus*, transforms this genus into a species of *Hoplocrinus* with four or five, as the case may be, arm-bearing adials. However, it is scarcely probable that the division of he plate actually took place; it is more likely that the right pper corner of the azygous plate was gradually absorbed by the adial. This view is corroborated by the small size and peculir shape of that radial in *Hoplocrinus*, and a comparison of his with the same plate in *Hybocrinus*, in which it has aborbed a greater portion, and attained somewhat more the hape of an ordinary radial. In the latter genus the upper left orner of the azygous plate has become divided off into a pecial anal plate. In figures 4–11, representing the azygous ide of some of the later genera, the posterior radial grows arger by absorbing more and more the azygous plate until his, and at last also the special anal plate, disappears entirely t the dorsal side.

In our analysis of *Bœrocrinus* we have taken the absence of he right posterior radial to be a fixed character. It may be, owever, that in the Erras specimen this plate, exceptionally, vas undeveloped, which is not improbable in such a low and arly type. In that case the Erras specimen would be an imerfectly developed *Hoplocrinus*, and only a link toward a till more simple form in which that feature is constant. We vill not undertake to decide, without seeing the specimens, vhether under these circumstances the Erras specimen would e specifically identical with *Hoplocrinus dipentas*; we might ay, however, that there is apparently no serious objection to t, and this shows how closely the two types run together, and hat *Bœrocrinus* is not that aberrant "altogether different ype" which it was regarded by Carpenter.

The accompanying figures show the arrangement of' the plates of the azygous side in the principal genera of the Cyahocrinidæ.



ig. 1, *Bœrocrinus*; fig. 2, *Hoplocrinus*; fig. 3, *Hybocrinus*; fig. 4, *Iocrinus*; fig. 5, *Dendrocrinus*; fig. 6, *Homocrinus*; fig. 7, *Cyathocrinus*; fig. 8, *Poteriocrinus*; fig. 9, *Eupachycrinus*; fig. 10, *Ceriocrinus*; fig. 11, *Erisocrinus*. The letter *b* means basals; *a*, azygous plate; *r*, posterior radial; *x*, special anal plate; *t*, first plate of the anal tube, more generally known as third anal plate.

In *Iocrinus* (fig. 4), the arrangement is somewhat like that of *Hoplocrinus*. The right posterior radial is almost as small as in that genus, but in place of occupying the corner only, it extends over the whole width of the azygous plate, from which it is separated by a horizontal suture. There is no special anal plate, the ventral tube rests upon the left sloping side of the radial, and as the tube has no connection with the azygous plate, we suggest that possibly the radial may embrace an undivided anal piece.

In *Dendrocrinus* (fig. 5), the case is similar to *Hybocrinus*, but the azygous plate is comparatively much smaller than in that genus, being largely replaced by the anal plate which at one side extends down to the line of the basals. The posterior radial and the azygous plate combined resemble in form the radial in the four other rays, being of about equal size and separated by a horizontal suture.*

In the Upper Silurian *Homocrinus* (fig. 6), the azygous plate is reduced to a small quadrangular piece, resting obliquely against the right posterior radial, against the anal plate and two basals. The anal plate is large and connects with the basals, and these touch the adjacent radial.

In *Cyathocrinus* (fig. 7), which appears in the Upper Silurian and survives the Sub-carboniferous, the azygous plate, which in *Homocrinus* had been already greatly reduced in size, is absorbed wholly by the posterior radial, and there is left only a large special anal plate. In this genus, contrary to the preceding ones, and those of other Cyathocrinidæ which have an azygous piece, the right posterior radial has the same form and size as the one toward the left.

In the preëminently Sub-carboniferous genus *Poteriocrinus* (fig. 8), we find an azygous plate, but this is pushed out still more from beneath the radial than in *Homocrinus*; its form is pentagonal, owing to an additional plate, which rests upon its upper truncate side. This plate, which is hexagonal and best known as the third anal piece, is actually a plate of the ventral tube incorporated within the calyx. The anal plate, which is smaller than in the two preceding genera, is hexagonal, and alternately arranged with the azygous plate and the plate above.

In the typical *Eupachycrinus* (fig. 9), the case is almost the same as in *Poteriocrinus*, only the azygous plate is comparatively more developed. This is somewhat remarkable, as *Eupachycrinus* is one of the latest known forms of the Palæocrinoidea, and, apparently even within the limits of this genus, or contemporaneously with it, we find all the gradations, from the most extravagantly developed anal side to its simplest forms. Aside from the irregular symmetry, which evidently represents a retrogression, the species show in all respects that they belong to one of the highest types of the group, and particularly in their mode of articulation they are fully up to the Jurassic Neocrinidea.

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* In Revision I, pp. 65–75, we considered the combined right posterior radial and the azygous plate in *Dendrocrinus*, which in their position and proportions resemble the right posterior radial in *Cyathocrinus*, to be a compound radial. At that time we thought that the second, the so-called azygous plate, in *Dendrocrinus*, *Homocrinus*, and in the Cyathocrinidæ generally, was a modified radial, and also that the anal tube, possibly, had been developed from an arm. Upon these points we were evidently in error.

In fig. 10, a form which has been generally included with *Eupachycrinus*, but has been very properly separated by Dr. White under the name *Ceriocrinus* (Contributions to Palæontology, No. 6 (1880), p. 127), both the azygous and the special anal plate are absent, and only the small third anal retains its usual position, which extends partly beyond the limits of the radials.

In *Erisocrinus* (fig. 11), one of the last survivors of the Cyathocrinidæ, the calyx is perfectly symmetrical, the azygous and anal plate being entirely absorbed by the other plates.

The different phases in the Paleontological development of the azygous side to which we have here alluded resemble most remarkably the stages of growth in the anal arrangement of *Antedon* during its Pentacrinoid life. At a time when even the radials were yet imperfectly developed, we find in both forms a large anal (azygous) plate (*Bærocrinus*), which is lifted out from between the radials (*Hybocrinus*) and becomes developed into a conspicuous funnel (the later Cyathocrinidæ), until at the termination of Pentacrinoid life and the close of the Carboniferous, the anal plate disappears entirely. The gradual disappearance of this plate in the growing animal is represented paleontologically by the modifications which take place in the form and size of the plate, and these modifications represent important generic characters. It must be stated, however, that the "anal" plate of the young *Antedon* is evidently not the homologue of the plate in the Cyathocrinidæ which we have designated as the "special" anal plate, but that it is the equivalent of the undivided azygous plate in *Bœrocrinus* and *Hoplocrinus*. We further hold that the special anal plate in *Hybocrinus* is the first step toward a plated tube which in that genus is reduced to its minimum size, consisting of only a single plate. The diminuitive tube possessed in its crenulated, rhomb-like structure an organization similar to that of the later genera in their large porous sac composed of numerous plates, and it is probable that both structures performed the same functions, and these may have been identical with those of the rhombs in the Cystidea.

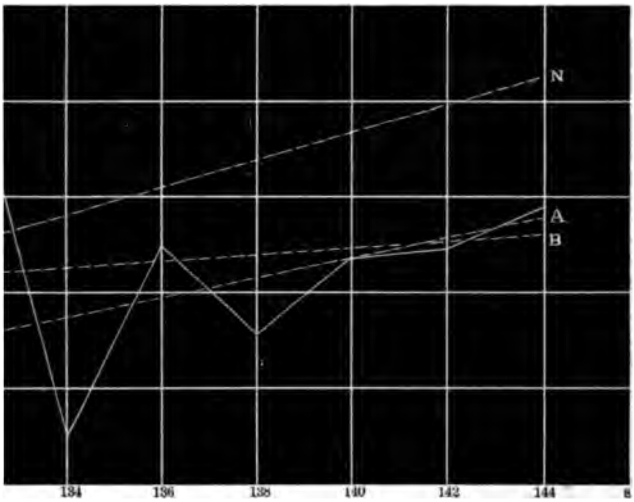
ART. XLII.—*Evolution of the American Trotting Horse*; by W. H. PICKERING.

THE two articles which have appeared on the above subject in the July and August numbers of this Journal have but just come to my knowledge, as I have been absent from the city for several weeks. Mr. Nipher has dealt with the question in a somewhat different manner from myself (see Science, May 4th), and has arrived at quite different results.

According to my calculations the value $\frac{ds}{dT}$ (s = seconds, T = years), instead of diminishing was nearly constant, with a slight tendency to increase. Or, in other words, horses were improving their speed slightly more rapidly now than they were a few years ago. This of course might readily be explained by the greater care bestowed in breeding and by greater attention being paid to the subject now than formerly. This could not last for many years, however, and the value of $\frac{ds}{dT}$ must finally diminish to zero. But in the mean time we have no means of saying how soon this will take place, or what will be the value of s. According to my work we could only predict what speed would be made in the course of the next few years, and it was concluded, for example, that two minutes would be reached about the year 1907.

Instead of taking his dates directly from the races, Mr. Nipher prefers to interpolate them from some curves which he has calculated. If all the observations contributed by Mr. Brewer could be included in this scheme, it would undoubtedly be the best method. As it is, it limits our author confessedly for accurate work, between the dates 1854 and 1873, whereas, the direct method is capable of including all observations from 1824 up to the present time. It therefore seems to me that the greater number of observations more than compensates for the greater accuracy of the few. And indeed I think this is indicated by the fact that we shall be able to draw quite a different conclusion from our author, though using the results obtained by his own method, while, by using all the observations as in my former article, I think but one conclusion can be drawn.

At the top of page 22 (July number), Mr. Nipher gives a table, the third column of which $\frac{ds}{dT}$ he calculates from alternate differences in the first two columns. Now where there are so few observations I think this method is hardly of sufficient accuracy, and that it is somewhat better to divide the ifferences of the first column, by those of the second, y. By so doing the figures in the third column will), 55, 54, 46, 55, 35, 75. This method does not smooth he curve at all, but shows it with all its original irregu. Plotting now a curve with these figures as ordinates, e corresponding values of *s* as abscissae, we get the ng figure:



e results could evidently be represented by a curve. ey show what mine did, only I think in an exaggerated he increasing value of $\frac{ds}{dT}$ within the last few years. pher, however, prefers to represent them by a straight This line N is shown upon the plate, calculated from his of *a* and *b*. Following him in this respect, two lines of n A and B are also given. Now there are two methods ich we may determine approximately the accuracy with a line represents a series of observations. First, that he line coincides as nearly as possible with the points ed, the algebraic sum of the ordinates should be zero; ond, that the sum of the squares of the ordinates should uced to a minimum. Making these calculations we obe following results:

	N	A	B
Sum of the ordinates	72	15	1
Sum of the squares	1900	1073	931

In both particulars we find that B best represents the results, and it is the line having the least inclination. We therefore conclude that $\frac{ds}{dT}$ is diminishing very slowly. Calculating its equation we find $\frac{ds}{dT} = {\cdot}084 + {\cdot}0033\ s$. According to this, $\frac{ds}{dT}$ can only become zero when $s \doteq -25$, or in other words the speed of the trotter will only cease to improve when he trots a mile in minus twenty-five seconds, or in less than no time at all.

I therefore maintain that Mr. Nipher's method, if correctly interpreted, gives in general the same result as my own. By it we may foretell what will be the speed attained for a few years in advance, but we cannot tell what will be the ultimate speed, nor when it will be reached.

Institute of Technology, Boston.

Art. XLIII.—*On the Discovery of Utica Slate Graptolites on the west side of the Hudson a few miles north of Poughkeepsie;* by Mr. Henry Booth.

During the construction of the West Shore Railroad, on the west bank of the Hudson River, the large number of cuttings made afforded a good opportunity for examining the slate and limestone rocks between West Park and Cornwall.

At two points small beds of Graptolites were found by Mr. C. Lown, of Poughkeepsie, and myself. The localities were nearly obliterated by the workmen, but we obtained some specimens which were referred to Professor R. P. Whitfield, of New York. Enclosed is his letter, giving the names of the graptolites submitted to him.

The localities are, one at Blue Point, about two miles south of New Paltz Landing (now called Highland), and one about a mile north of the same place.

Poughkeepsie, N. Y., Sept. 29th.

Copy of Letter from Mr. R. P. Whitfield, dated American Museum, New York, August 28th.

I find among the Utica Slate Graptolites sent me, *Diplograptus pristis* Hall (not of Hisinger); *Climacograptus bicornis* Hall; *Dichograptus furcatus* Hall; *D. divaricatus* Hall?; *Monograptus gracilis* Hall; *M. sagittarius* Hall, and *Diplograptus marcedus* Hall?.

ART. XLIV.—*Becraft's Mountain;* by WILLIAM MORRIS DAVIS.

Previous opinions concerning this locality—Formations occurring here—Their absolute and relative positions—Question of nonconformity between Lower and Upper Silurian—Notes on the accompanying map—Relations of Lower and Upper Silurian elsewhere.

BECRAFT'S MOUNTAIN is a small, low outlier of Lower Helderberg limestones, resting on the Hudson River formation, two miles east of the river and about a mile southeast of the city of Hudson, Columbia county, New York. If the name apply to all the limestone area, it would include an oval mass nearly two miles long, north and south, and over a mile wide, low on the ends but bordered with strong bluffs from fifty to one hundred and fifty feet high on the sides. The interest in the locality arises not so much from the isolated position of the limestones, as from the reported nonconformity between the strata of Lower and Upper Silurian date. The previous descriptions and figures of the mountain are collected in my paper on the Folded Helderberg limestones east of the Catskills (Bull. Mus. Comp. Zool., Cambridge, Geol. Series, i, 318, 1883); it will here be sufficient to mention that Mather, Rogers and Dana described it as showing an unconformity, while Emmons showed the contact of the upper and lower rocks conformable, and Hall was disposed to take this view. The figures given by Mather are very rough, and the one (Geol. N. Y. 1st Distr., pl. 24, fig. 6) showing a complete cross-section, seems to be generalized on a very theoretic basis.

During a recent recess in college work, I took opportunity to visit this locality with a party of students, to see how clear the evidence might be in favor of one or the other of the above views, and in two days spent on the ground obtained the following results.

The formations occurring in this region are, beginning with the oldest:—1. The so-called Hudson River group; shales and shaly sandstones, with much fine jointing or rough cleavage that might often be well mistaken for bedding; sometimes showing clear bedding as well, which is then generally even, though in some outcrops it seemed very irregular and was there flinty and indurated. The outcrops were very few compared with those of the overlying limestones, as heavy terraces of stratified drift surround the base of the hill. No fossils were found in this formation, and in using the name "Hudson River" I simply take that which is most generally accepted.

2. The Lower Helderberg limestones, consisting of:—

2*a*. Tentaculite limestone, probably including the Ribbon limestone and possibly some equivalent of the Water-lime; fine, blue, even, thin beds, weathering light-colored and smooth. A few imperfect *Leperditiæ* were found in it. Thickness, twenty to thirty feet. No representative of the Coralline limestone was noticed at the base of this subdivision.

2*b*. Lower Pentamerus limestone; heavy, knotted, grayish-blue beds, with irregular nodules of chert. Fossils were not common, but *Pentamerus galeatus* and *Atrypa reticularis* were found at several places. Thickness, forty to fifty feet.

2*c*. Catskill Shaly limestone; deep blue, weathering brown and shaly; generally showing its many fossils as casts. *Spirifer macropleura*, *Hemipronites radiata* and *Strophomena rugosa* being most characteristic among many others. Thickness, fifty to sixty feet.

2*d*. Upper Pentamerus limestone, probably with representatives of the Encrinal or Scutella limestone; a hard, gray, crystalline limestone, largely made of fragments of shells in somewhat even layers. *Spirifer medialis*, *Orthis* ——— and *Pentamerus pseudo-galeatus* are most easily identified. This rock is quarried as 'shell-marble,' and is cut by steam drills into large blocks as much as five by five by twelve feet; it is also burnt for lime, and used in smelting iron ores at Hudson. Thickness, forty to fifty feet.

3. Cauda-galli shales; barren, gray, monotonous shales, seldom showing any trace of bedding, but breaking on an irregular, nearly vertical cleavage. No fossils. Thickness, one hundred and fifty to two hundred feet. This formation has not been previously mentioned as occurring here.

4. Corniferous limestone; a hard, cherty limestone lying on the Cauda-galli shales over a small surface, and doubtfully referred to this formation. No fossils. Thickness, ten to fifteen feet.

5. An unknown conglomerate of limestone fragments in a compact siliceous matrix. The fragments were tolerably well rounded, and seem to correspond with the several subdivisions of the Lower Helderberg. Only two localities of this deposit were found, both of them outside of the bluffs; one making a low mound in the meadow at the left end of section I; the other in the irregular mounds (F on map), a quarter of a mile south of the southern limiting road. At neither point could the position of the bedding be determined, and no conclusion was reached as to the age of the deposit, except that it is apparently younger than the Helderberg series and certainly much older than the drift. It is apparently to this that Emmons makes reference (Agric. N. Y., i, 1846, 136).

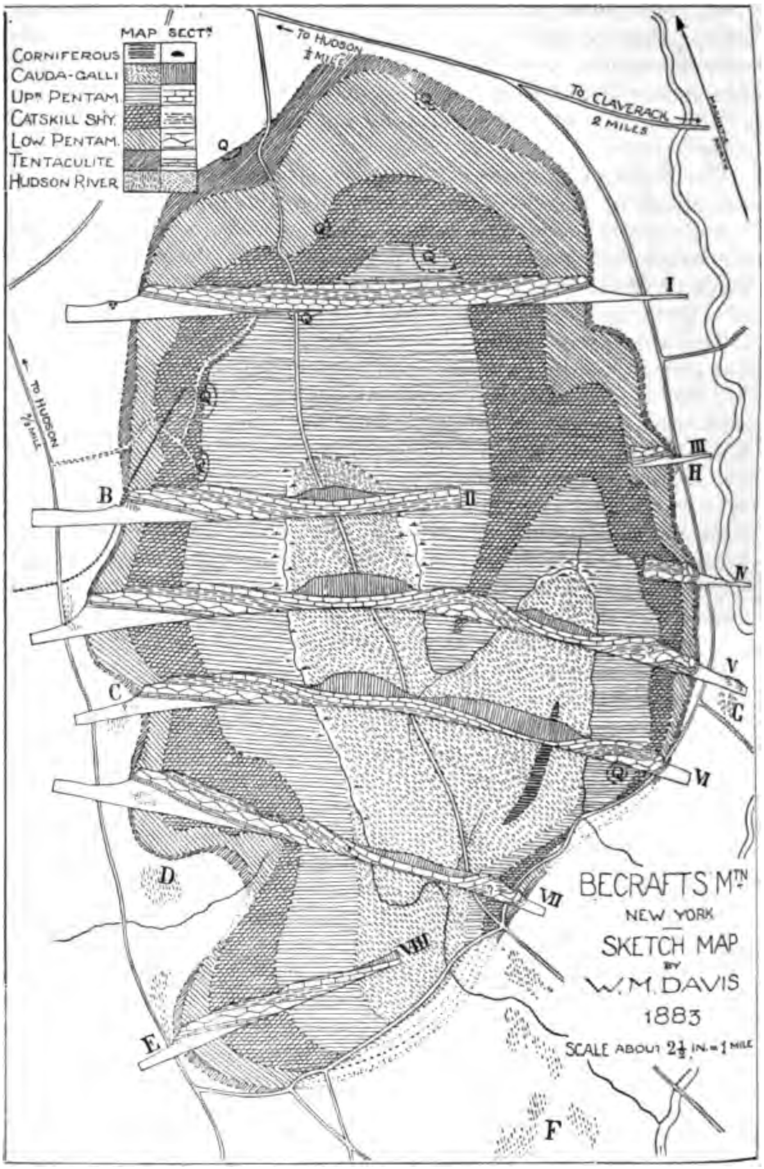
6. Drift; seen chiefly in the stratified sands of the terraces

at are two or three hundred feet above the river. Erratics ere commonly quartzites and granitic rocks, often garnetifous; some of these were scratched. None of the ledges were riated, nor could we expect to find them so, as all had been ng exposed and were well weathered.

The position of the several formations may be described as llows:—

The Hudson River strata have as a rule a regular northerly rike east of the river, and are generally found with a steep sterly dip; at several points a few miles to the west and uthwest they rise in ridges of greater height than the crest of ecraft's Mountain, but on the east, their surface is lower and seldom exposed on the railroad between Hudson and Chatham.

The limestones form a flat synclinal, about a mile and three-uarters along the axis and over a mile broad, with the middle ne trending N. 20° E. (magnetic bearings given in all measres), and the greater heights on the western side. On the east, nall subordinate folds are added, each one more distinct on e southeast and fading away to the northwest; and, when ken together, showing an increase in sharpness toward the utheastern border, where they follow one another in rapid ccession, always with the steeper pitch of the anticlinal on e west, and with overthrown dips at several points. The istinctness of this small imitation of Appalachian structure is rprising. Excepting at the northern and southern ends, here the margin of the limestone is somewhat doubtfully ferred, the outcrops always appear in the form of pronounced luffs, rising on the southwest where highest about two hundred et above the terrace plain. The Tentaculite and Lower Penmerus generally make the outermost and strongest bluff gether; the Catskill shaly limestone and the Upper Pentaerus form roughly concentric ridges at variable distances ward from the margin; the Cauda-galli shales make smooth, und hills southward from the middle of the oval area within e bluffs. The more complicated structure of the southeastern de is not clearly revealed in its form, and requires close obseration of dip for its determination. I am not sure that there ould not be one more small fold there than is represented in e sections. There is no sufficient evidence of faults such as ather shows on both his sections. The axis of the general ynclinal dips gently to the south; at its northern end the mestones lie essentially horizontal; but at the southern, where xposed, they are turned up abruptly so as to stand on end ith a strike varying from W.—E. to N. 70° E. This abrupt lding may be torn into a fault on part of its length as the mestone outcrops are wanting on much of the distance where ey should appear.


MAP SECT^N
CORNIFEROUS
CAUDA-GALLI
UP^R PENTAM.
CATSKILL SH'Y.
LOW PENTAM.
TENTACULITE
HUDSON RIVER
← TO HUDSON ½ MILE
TO CLAVERACK 2 MILES
← TO HUDSON 7/8 MILE
Q
I
II
III
IV
V
VI
VII
VIII
B
C
D
E
F
H
BECRAFTS M^TN
NEW YORK
SKETCH MAP
BY
W. M. DAVIS
1883
SCALE ABOUT 2½ IN. = 1 MILE

The relative position of the Hudson River strata and the overlying limestones is the point of most importance and interest, and the weight of evidence here seems to be in favor of nonconformity, though this relation is not nearly so well shown here as in the remarkable exposure in the old cement quarries at Rondout, on the other side of the Hudson, about thirty miles farther south.

The following observations show what was found at all the outcrops below and in the neighborhood of the limestone bluffs.

A. In a cut on an old quarry railroad, a quarter of a mile west of the limestone bluffs (not included on the map); shaly layers, strike N. 25° E., dip 35° to E.S.E.

B (see map). In another cut of the same old railroad, directly under the western bluff, as the road rises obliquely along the slope to reach the quarries; shaly outcrop, N. 55° E., dip 25° o S.E. A few hundred feet farther up grade or northeast, the cut shows the lower layers of the Tentaculite limestone, N. 35° E., dip 15° S.E. This variation of position is not significant, for equally great variations are found in the limestone tself in passing short distances along the cliff face at many points. The cut here reveals a considerable depth of talus fragments, often including large blocks that might be taken for edges in place if not fully exposed, and show how difficult it would be to find a natural contact underneath the bluffs.

C. In a little stream channel leading down from a depression n the bluffs, a few hundred feet south of a glue factory; the Tentaculite stands N. 23° E., dip 15° E.S.E., at base of limestone outcrops, and about twenty feet from it the shale appears, but unfortunately shows nothing but cleavage, N. 25° E., dip 50° E.S.E.; however tempting it may be to assume that the cleavage and bedding coincide, there is not the least evidence that they do. The steeply inclined splitting of the horizontal Cauda-galli shales within the limestone synclinal shows how entirely untrustworthy this appearance may be as a guide to the position of stratification. On the road-side, about half way from B to C, the bedding was N. 25° E., dip 30° E.S.E.

D. On a mound on the northern side of the entrance to a deep valley near the southwestern end of the mountain. The imestones are here nearly horizontal; no contact is found, but about forty feet down the slope from the lowest visible layer of the Tentaculite, there are numerous small outcrops of a very uneven, indurated, flinty rock, for which no position could be determined satisfactorily. Its irregularity would, however, ndicate unconformity. Similar small patches of indurated slaty rock, from which the bedding has disappeared, are found near the road on the other side of this valley.

E. At the southwestern end of the mountain, a small opening on the road-side exposed the Tentaculite with a faint easterly dip lying with smooth contact, about ten feet long, on hard slaty rocks, in which the appearance of bedding was N.—S., dip 50° E. The face of the rocks was so weathered and jointed that it was difficult to make sure that the contact was a true unconformity and not a slip.

F. A third of a mile south of the limestones, several irregular mounds (a few outcrops of the limestone conglomerate were found on one of them) rising among the terrace flats show the same indurated, flinty outcrops as were found at D. Some beds were found standing N. 25° E., dip 40° E.S.E., and smaller outcrops continued toward the limestones, across the creek that flows out from the synclinal. These seem to show that the older rocks maintain a northerly strike where the limestones turn around toward the east.

G. A few hundred feet southeast of the outlier, the sandy shales rise in small ledges, with a tolerably well-defined bedding, N. 50° E., dip 60° S.E. This conforms fairly with the nearest limestone, if we suppose it to have an overthrown dip, as is not at all improbable.

H. A small cutting on the side of the eastern road uncovers shales with a faint easterly dip. About fifteen feet higher up the bank the lower limestones are inclined gently to the west. The difference between the two is very small, and is fully equalled by differences in the position of adjacent limestones a little farther south, where the folds are crowded together.

From this point, around the rest of the circumference, no outcrops of the older rocks were found, although the whole distance was carefully searched.

The observations thus detailed may be summarized as follows: A, C, and G are non-committal; if necessary they could agree with either conclusion. B and H, if seen alone, would be taken as decisive of conformity. D, E and F imply unconformity, but with nothing of the distinctness shown in Mather's section. It is notable that these three outcrops show a much more indurated and irregular rock than the others; and one could easily suppose them to belong to a series independent of and unconformably below all the others, shaly sandstones as well as limestones. Indeed, this seems to be the conclusion reached by Emmons. He wrote: "Another limited fracture appears on the southeastern side of Becraft's Mountain, about three miles southeast of Hudson. On one side the Taconic slate appears supporting a fragmentary mass of Calciferous sandstone; on the other, the inferior members of the Helderberg division, the thin bedded Water-limes and Pentamerus, beneath which are the gray sandstones of the Hudson river" (Agriculture of New York, i, 1846, 136). The evidence of such a fracture is indistinct, and the identification of the limestone conglomerate apparently at our locality F with the Calciferous is certainly hazardous. Fossils being absent and outcrops few and scattered, it is difficult to find observations that shall compel a single explanation. Perhaps further exploration of the other Helderberg outliers of which Mather makes brief mention may aid in solving the question.

A few words may be said concerning the accompanying sketch-map and sections. They are based on estimated distances and compass bearings taken all around the bluff outcrops, and on many zig-zag section lines within the limestone area. While the drawings cannot claim close accuracy, they nevertheless combine very successfully all the observations made on rocks, roads and streams. The sections are placed directly where they belong on the map, as more seems to be gained by showing them in their true positions than is lost by breaking the continuity of the map surface. The Helderberg rocks are constructed in their probable position underground, but the Hudson River strata are shown only at their outcrops. The part of least accuracy is on the southeast, where the folds produce more complexity than could be unravelled in the time we had on the ground. As already mentioned, the strip of limestone called 'Corniferous' is so named only because it is cherty and lies on the Cauda-galli shales; but the identification is very probably correct. The Hudson branch of the Boston and Albany railroad passes a little distance northeast of the map, within clear view of the Lower Pentamerus bluffs.

The relations of the Lower Helderberg to the Hudson River formation are somewhat peculiar in connection with the occurrence of Silurian non-conformity. When the full series of Silurian deposits is represented, the two groups in question are separated by several intermediate formations—the Oneida, Medina, Clinton, Niagara (these four belonging to the Niagara period) and the Salina—which attain a total maximum thickness of several thousand feet, and consequently imply the passage of a long interval of time. The only part of this interval marked by disturbance is between the Hudson River and the Oneida epochs, but this disturbance does not seem to require the absence of any known part of the series. The following review of contact-localities will show this. On the northern face of the Helderberg Mountains, the formations of the Niagara and Salina periods are essentially wanting, and the Lower Helderberg limestones rest on the Hudson River shales, separated only by a few feet of transition deposits, all the strata being horizontal; the junction here has always been described as conformable, in spite of the long time that passed without deposing its record. Whether the actual contact has been observed or not does not appear, nor has there yet been given any satisfactory explanation of what was going on here during the long unrepresented time. This conformity was found also near Catskill in the folded strata east of the Catskill Mountains, in Green county (see my paper above referred to). At Rondout, Ulster county, a fine, uneven unconformable contact is clearly shown in the old cement quarries, where the entire Niagara and Salina groups are represented by only about six feet of limestone; another is noted at Otisville, N. Y., near the Pennsylvania line, in a cut on the Erie railroad (see Second Geol. Surv. Penn., G 6, 150), where the intermediate strata are seen in some force and the Oneida conglomerate lies on the Hudson River slates. Farther southwest, in Pennsylvania, fragments of the slates are found in the Oneida, and hence nonconformity is inferred, although the sudden variation of dip seen at some points on the southeastern slope of the Kittatinny Mountain that has heretofore generally been explained as a nonconformity, is now regarded by Professor Lesley as the effect of a fault (l. c.). In eastern Tennessee, the Lower and Upper Silurian are perfectly conformable, and the series of strata is fairly complete before and after the critical date. In southern Ohio, an uplift and erosion are described as occurring at the beginning of Upper Silurian time; and a similar but longer disturbance is reported in western Tennessee; but in both these localities, the older and newer strata are essentially parallel. Conformity and nonconformity are therefore quite independent of the presence or absence of certain members of the series; and absence of the upper members is independent of distinct dislocation of the lower members. With a tolerably full sequence of strata about the time of disturbance, there is normal conformity in eastern Tennessee, and nonconformity in southeastern New York and northeastern Pennsylvania; on the other hand, there is conformity in the Helderberg Mountains, and nonconformity at Rondout, although the intermediate strata are essentially absent at both these localities. Again, the strata are essentially horizontal and parallel in the Helderbergs, where the series is incomplete and undisturbed, as well as in southern Ohio, where the series is fuller, but where fragments of the lower are found in the upper members. Finally, the strata are folded and conformable with lapse of intermediate members at Catskill; folded and unconformable with similar lapse at Rondout; folded, unconformable and without lapse farther southwest. Other examples could be quoted from the Canada surveys. All combinations of these various elements appear, and there is therefore no necessity of supposing unconformity at Becraft's Mountain simply because the limestones there do not belong immediately after the shales in the geological series. Moreover, the generally flat position of the limestones and tilted position of the shales east of the Hudson does not decide the question, for the shales are twice seen almost flat under the limestones of the outlier, and on the southeastern side the limestones are strongly tilted. The suggestion above that the shales and slates are of different ages requires much further observation for its proof. This indefiniteness of indirect evidence is the more unsatisfactory from the incompleteness of the contact outcrops; and as it is very possible for an appearance of nonconformity to arise in a series that is all folded at the same time, on account of the unequal folding of adjoining strata of different resistances, nothing but direct and clear exposure of an uneven and surely unconformable contact will suffice finally to settle this point in the structure of Becraft's Mountain.

Cambridge, April 23, 1883.

ART. XLV.—*The Nonconformity at Rondout, N. Y.*; by W. M. DAVIS.

Description of the formations occurring here—Upper Silurian unconformable on the Lower—Altitude of the rocks and local topography—Glacial and surface geology.

THE reference made in the preceding article on Becraft's Mountain to the clear evidence of the unconformable relation of the Upper and Lower Silurian rocks at Rondout on the Hudson suggests the publication of a more detailed illustration of this noted locality than has yet appeared. The accompanying map and sections are prepared with this object from notes taken during an excursion in April of this year, and with a short descriptive text these may serve as a supplement to the earlier papers by Lindsley* and Dale.†

The formations occurring here are in most respects like those at Becraft's Mountain and in the limestone belt west of Catskill, which I have already described;‡ the following notes will point out their local peculiarities. The Hudson River rocks are sometimes heavy-bedded sandstones of rather fine texture and considerable hardness, as in Huzzy's Hill, a ridge of several hundred feet elevation two miles south of Rondout; and sometimes shaly and much contorted, as along the banks of the stream at Eddyville. They occupy all the area east of Rondout Creek, with small exception, but are generally hidden under terrace rift. The Oneida conglomerate is represented by a stratum of almost quartzitic texture, white or bluish in color, and fifteen to twenty feet thick. The most northeastern point where it was certainly recognized was in New Salem at the beginning of the horse-railroad leading to the cement quarries in Whiteport; thence southwestward it may be frequently seen at the foot of the limestone bluff, a hard, compact bluish quartzose rock. At the foot of the map, where the limestone ridge breaks down, it is white, and so appears again about two miles west in the fine anticlinal near Whiteport where the Water-lime has been deeply quarried. It holds no pebbles or fossils here and is not yet thick enough to have a noticeable effect on the topography.

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* Poughkeepsie Soc. Nat. Sci. Proc., ii, 1879, 44–48.

† This Journal, xviii, 1879, 293–295. Dale's figures belong near section II on our map.

‡ l. c., and Bull. Mus. Comp. Zool., Geol. Series, i, 1883, 311–329.

The first of the limestones is seen in the quarry on the hill overlooking Rondout (Section II), where it rests directly on the Hudson River rocks, as will be further mentioned below. As measured by Mr. J. G. Lindsley, superintendent of the Newark Lime and Cement Co., it is six to eight feet thick, and from its abundance of corals it is considered the equivalent of the Coralline limestone of Schoharie, the thin eastern extension of the Niagara of Western New York.*

The Water-lime and Tentaculite, to which Lindsley adds the Stromatopora and Ribbon limestones, have a total thickness of seventy feet. The layers are generally smooth and even, especially in the upper part and are readily recognized, but fossils are not common. The lower beds are deeply quarried at numerous points in this neighborhood for cement-rock, of which a great quantity is burned, ground, barreled and shipped. The annual yield has in the past few years risen to 2,000,000 barrels, valued at about a dollar a barrel. The openings thus made give excellent views of the deep plunging of the strata into the earth, especially at the Whiteport anticlinal above referred to. At Catskill twenty-five miles north of Rondout the rock is not found adapted for hydraulic cement; and though therefore not economically *a* water-lime, I am confident that it is geologically *the* Water-lime. The Lower and Upper Pentamerus limestones, about thirty and forty feet thick respectively, generally retain the characters seen at Catskill and Becraft's Mountain; the Lower being heavy, uneven, cherty, with comparatively few fossils; the Upper being also heavy and somewhat uneven bedded, but rich in fossils and free from chert. These are separated by the Catskill shaly limestone, about sixty feet thick (Lindsley gives it only sixteen, which seems much too little), and overlaid by a similar formation, certainly over one hundred feet thick and probably as much as one hundred and fifty. These I have mapped as the Lower and Upper Shaly limestones; they are generally very impure, and hardly deserve the name of limestones; their outcrop is very ragged and the Upper Shaly sometimes weathers on cleavage instead of on bedding planes, giving a very deceptive appearance (as on the southwestern synclinal ridge); in spite of its shaly texture, it stands higher than the limestone below it. Fossils are not nearly so numerous as in the Shaly limestone at Catskill; but it is interesting to note that several characteristic species (*Spirifer macropleura*, *Strophomena rugosa*, *Hemipronites radiata*) common to both the Lower and Upper Shaly beds are absent or very rare in the intermediate Upper Pentamerus, and thus seem to confirm the supposition that both these shaly deposits were formed comparatively near the old shore line, which retreated eastward carrying its fauna along with it and so allowed the pure Upper Pentamerus limestone to form, and then came back again with its fauna unchanged, driving the deposition of pure limestone away to the west. The Upper Pentamerus is often deeply quarried for lime, as at the several points where Sections I, II and III cross it. The passage from one to the other of the above subdivisions of the Lower Helderberg limestones is comparatively gradual; but an abrupt

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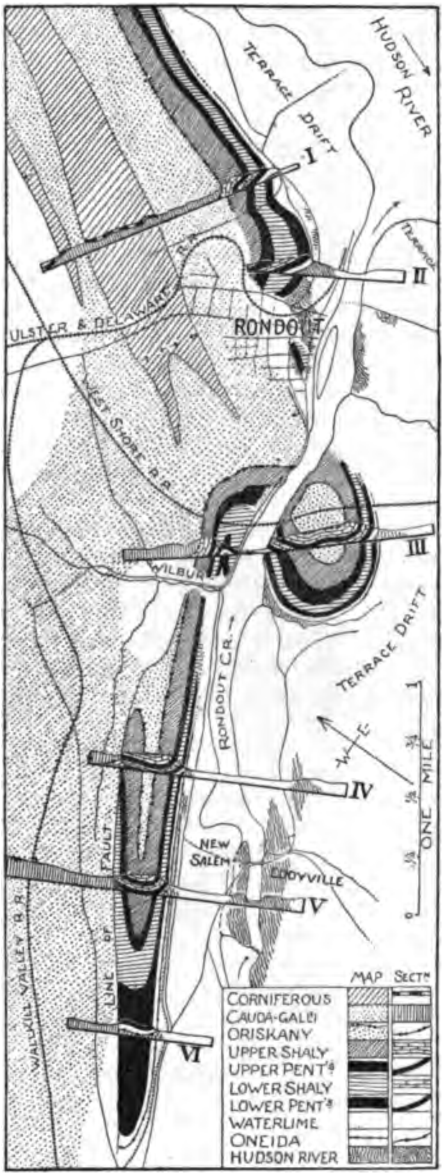
* See J. Hall, Pal. N. Y., iii, 26, and Amer. Assoc. Proc., xxii, 1873, 321.



change at the top brings in the Oriskany sandstone and grits, about twenty and thirty feet thick, but variable. The lower member is sometimes absent, but when seen it consists of pure white quartz, sometimes dense, sometimes rather loosely built of subangular white pebbles up to one-third of an inch in diameter, without fossils. This is followed by a hard grit with casts of numerous fossils, among which *Spirifer arenosa* was most frequently found. As usual, the stone fences give the best collecting ground; for example on the ridge northeast of the West Shore bridge across the Rondout, and on the southwestern synclinal. Above this, we find the barren Cauda-galli shales, lifeless but for the occasional sea-weed imprints; fine textured and generally with the bedding completely replaced by the characteristic steep cleavage plane. Its thickness is very probably three hundred feet or more. A fine exposure of these shales is found in a bold synclinal bluff advancing toward the creek between Rondout and Wilbur; from here to the railroad bridge, a good downward sequence of formations is exposed. The Corniferous limestone, recognized only by its fine texture, plentiful chert, and position, completes the series.

The interruptions in the sequence of formations at Rondout are of two kinds. One, that may be called passive, consisted essentially in withholding the supply of sedimentary material, so that certain epochs were left unrepresented: thus the Medina, Clinton and Schoharie deposits are completely wanting, and the Oneida and Niagara are almost absent. The other interruption came between the deposition of the Hudson River and the Coralline or Oneida strata and was of a much more active character; the Hudson River rocks were upheaved, exposed to erosive forces, presumably of the ordinary subaerial kind, and then submerged to receive the later formations on their worn edges. This is most excellently shown in the old quarry in the hill overlooking Rondout (at Section II) where the contact line has been perfectly exposed for ten feet or more; a fair inference of nonconformity

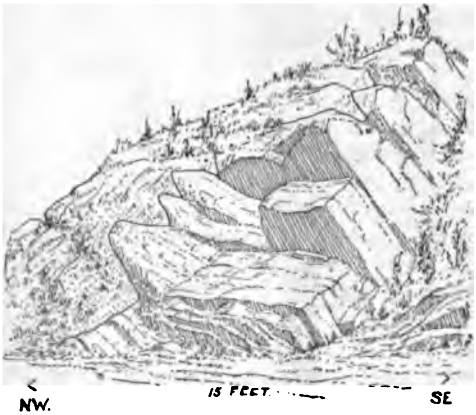


Fig. 1.

can be made at several other points along the slope. The rocks on the western side of the limestone outlier, opposite Wilbur, also give good evidence of non-conformity, but we found no contact there. This locality is described and figured by Mather. The irregularity of the contact in the Rondout quarry completely excludes the possibility of the disagreement of dips in the over- and underlying formations being due to a fault, at least at this point, for as shown in figure 1, the limestone fits closely into channels worn along the strike of the softer sandstone beds, and the two rocks are so firmly consolidated that hand specimens can be easily obtained showing the line of junction. It is noteworthy that the limestone begins immediately with its fully determined calcareous character: there is no band of transitional composition; no fragments of the sandstone are contained in the overlying rock. The old worn surface was swept clean before the corals and crinoids began growing upon it, and their fragments and grindings make the first deposit. Some little pieces of crinoid stems lie directly on the bare sandstones. From the absence of the Oneida sandstone here, it may be inferred that the Hudson River rocks remained out of the Paleozoic ocean longer at this point than they did two miles to the southwest of it, where the earlier Upper Silurian strata lie directly upon the Lower Silurian beds. At the end of the quarry-railroad in Eddyville, already mentioned, a contact is opened for nearly one hundred feet, but unlike the one at Rondout, it is even, and the rocks agree in strike if not in dip. It is therefore probable that this contact is not the original one, but has been produced by faulting or slipping when the rocks were folded. The possibility of such slipping is well shown in the deep quarry (Newark Lime and Cement Company) a little south of Section I, where the Water-lime is folded and slipped so as to be apparently unconformable to itself. The probability of such an occurrence at Eddyville may be inferred from the sharp folding that occurs all along the front of the limestone bluffs. On the other hand, it is also probable that, if an original contact could be seen, it would be truly unconformable, for this relation occurs on both sides of it; at Rondout on the northeast, and at Otisville on the Erie railway,* some forty miles southwest. At some later date, presumably contemporaneous with the great Appalachian disturbance, the entire series of rocks described above were folded and dislocated. The most pronounced effects of this deformation are now seen along the bluffs of Lower Helderberg limestones. Beginning at the northeast near the Hudson, the rocks either stand on end or dip strongly to the west; they fall to a less angle on approaching Rondout and then are for a time more or less hidden in passing through the town. Crossing the creek, they build the limestone outlier, a flat synclinal with steep or vertical dips on the eastern side, and then cross the stream again by the railroad bridge and plunge underground at a high angle. The anticlinal half of this S-shaped curve is not a simple turn, but is complicated by a small additional fold where the stream crosses Section III, as is shown on a larger scale in figure 2.

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* Geol. New Jersey, 1868, 135. Second Geol. Surv. Penn., G 6, 150.

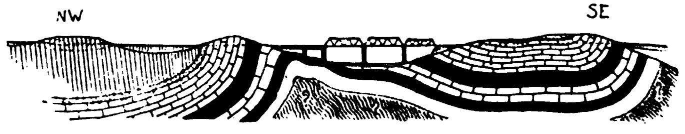

Fig. 2.

From here, up the valley, the strike is very uniform, and the bluff-front very even. Sections carried across it at several points showed a well-marked synclinal structure bounded on the northwest by a down-fault of moderate throw. In conformity with what seems to be " normal" for the Appalachians, this fault is drawn with the upthrow *overlying* the hade. By following along the axis of this synclinal from Wilbur, one passes over seven different subdivisions of the Upper Silurian that successively come to an end in the rising trough.

This detailed statement of the altitude of the rocks seems necessary in order to contradict the supposition not unfrequently made some years ago that a "great fracture" passed along the Hudson valley between the older and newer rocks. Apart from the folds and local faults of small throw there is no evidence whatever of any great disturbance, and the great fracture is purely hypothetical. The Oneida and Medina lose the mountain-making strength they possess in Pennsylvania because they thin out, not because they are cut off. The Cauda-galli shales cover a broad hilly surface back of the limestones, and in the northern part of the district examined they are overlaid by the Corniferous limestone. The blank area on the map where the three railroads meet is a Corniferous synclinal possibly holding some Marcellus shale, but filled with the drift sand and clay on which Kingston is built: Esopus Creek enters this plain from the west and follows it for some miles northward; and farther on, the arch (or fault) which forms the western side of the synclinal, disappears, and the Corniferous and Marcellus trough is continued as a Marcellus monoclinal valley, occupied by the Kaaterskill on its northward course and crossed by the Catskill at Leeds.*

Glacial striæ were observed at several places with a direction

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* See papers by the author in Appalachia, iii, 1882, 20; and Bull. Museum Comp. Zool., Geol. Series, i, 1883, 328.

S. 15° to 25° W. Unstratified drift, striated loose stones and erratics are uncommon; the most notable of the latter was found on the line of the West Shore railroad about one mile below the creek; it was a broken bowlder of a dense conglomerate in which the subangular pebbles were of jasper and quartzite.

Terrace flats cover most of the exposures at low levels, except where the creeks have washed them clean. In cutting out its path since the time of high water level when its old valley was much obstructed by sands, the Rondout has very clearly formed a new channel in the contorted Hudson River shales at Eddyville, the old channel being nearer the limestones. There is some appearance of another old channel south of the limestone outlier opposite Wilbur, but the present valley there is rather large for post-glacial cutting at so low a level.

The points of special interest that may be seen in one or two days' visit to Rondout are: first, the limestone bluffs and their deep quarries along the river, especially the quarry near the town showing the unconformity; second, the complication of synclinal and anticlinal on the line of the West Shore railroad; third, the new channel at Eddyville and the limestone synclinal opposite to it. A large quarry is opened on the river bluff a mile north of the map, and several others in a very interesting region between Whiteport and Rosendale southwest of our limits. Two railroads and the river boats afford easy access to the town; local tugs are constantly running up and down the creek between Rondout and Eddyville; and the Wallkill Valley railroad serves for longer excursions.

Cambridge, May, 1883.

ART. XLVI.—*Notice of Agricultural, Botanical and Chemical Results of Experiments on the Mixed Herbage of Permanent Meadow, conducted for more than twenty years in succession on the same land;* by D. P. PENHALLOW. Phil. Trans. Roy. Soc., Part IV, 1882. 4to.

IN 1880, Messrs. Lawes, Gilbert and Masters issued their report upon the first part of this valuable series of experiments under the head of "The Agricultural Results." Part II, now before us, embraces the "Botanical Results" and forms a voluminous publication of some 232 quarto pages, containing many carefully prepared and very detailed tables, showing the results obtained. The entire volume will well repay careful study, and even a hasty glance is sure to impress one, not only with the vast amount of time and care required to obtain and work up such results, but also with the necessity for giving ample time to such work in order to avoid too hasty generalizations,—an error which is quite too liable to occur in this country at the present time, but which Messrs. Lawes and Gilbert, with characteristic caution, have avoided.

The experiments were commenced in 1856 upon thirteen plots of land, a number subsequently increased to a total of twenty-two. These plots were studied without manure, and under treatment with mineral and organic manures varying both in quantity and composition. "In the first years of the experiments it was observed* that those manures which are the most effective with wheat, barley or oats grown on arable land —that is with gramineous species grown separately—were also the most effective in bringing forward the grasses proper in the mixed herbage. Again, those manures which were the most beneficial to beans or clover, the most developed the leguminous species of the mixed herbage, and *vice versa.* It was further observed that there was great variation in the predominance of individual species among the grasses and also among the representatives of other orders."

"Indeed, in the second year, 1857, the differences in the flora were so marked, that a first attempt was then made to separate and determine the proportion of each separate species, in carefully averaged and weighed samples taken from several of the plots at the time the crops were cut. . . . In these early trials the samples were separated into: 1. Gramineous herbage, stems bearing flower or seed; 2. Gramineous herbage, detached leaves and indeterminate stems; 3. Leguminous herbage; 4. Miscellaneous herbage," but these were afterwards reduced to three classes by combining the gramineous in one. "In all only eleven grasses, three leguminous plants and nine plants of other orders were then identified in the samples, though undoubtedly many more were present; and under the head of 'Gramineous herbage, detached leaves and indeterminate stems,' in one case as little as fifteen per cent, and in another more than fifty-three per cent of the total was recorded. This result at once illustrates both the difficulty of the work and the great difference in the character of growth on the different plots."

"From year to year the plots became more and more characteristic, and in the seventh season, 1862, it was decided to undertake much more complete botanical separations." For this purpose a botanical assistant was employed to take charge of the work, but "on each occasion, whether of more complete or of only partial separation, from three to six boys have also been occupied in the work."

The samples were taken by a collector who followed directly after the mowers until the whole plot had been cut. Each colection thus made was well mixed on a cloth to prevent damage nd loss, and from such a lot an average sample weighing from n to twenty pounds was selected for final botanical analysis. he sample was weighed without delay and then spread out to ry, after which it was put away for future examination. "In e conduct of the separations, each worker had a small hand- l of the mixed herbage placed before him, which he separated to its various component species as far as he was able. From me to time the superintendent revised each of the so separated ortions. But there always remained an undetermined residue, hich varied in amount exceedingly, according as the herbage as simple or complex, stemmy or leafy, mature or immature, nd so on. These at first undetermined residues, after some eduction in the hands of the superintendent, were next separated into portions of different character by means of coarse ieves of various gauges, by which the examination and the dentification of the various components were much facilitated. till . . . the amount of finally undetermined residue varied ery considerably according to the description and the character f development of the herbage."

The details of observation were most carefully planned and horoughly carried out, and were of such a nature as to cover he ground very fully. In the botanical separations, however, he fact that there always remained an undetermined residue ould seem to imply that another method than the one em- loyed might have given more accurate results. Doubtless, Messrs. Lawes and Gilbert had their own good reasons for the pecial course pursued, though they do not appear, but if the eighed samples had been separated while yet fresh, the exact etermination of the species could then have been made at any uture time, with certainty of avoiding, very largely at least, an ndesirable element of error. Under such a course, there would ave been little or no breakage; the species could have been ore readily identified by unscientific assistants; the work ould have been accomplished with greater ease and rapidity, nd the undetermined residue would then have become very mall, or have quite disappeared. Again, in comparing the roduction of the plots with the character of the seasons, suffi- ient importance does not seem to have been attached to local eteorological conditions as determined in the immediate icinity of the experimental fields. In the tables given, the ain-fall is that recorded at Rothamstead, while the tempera- ures are all given as determined at Greenwich, and therefore ot applicable in more than a very general sense. As has been hown on previous occasions, too much care cannot be taken in ecuring a local meteorological record in immediate connection with experiments, where exact considerations of the influence of season are concerned.

It is interesting to note that, as the vegetation advanced under the influence of cultivation, the plants found on the plots came to represent a total of 22 orders, 63 genera and 89 species. The most numerous were naturally *Gramineæ*, of which there were 22 species. Next in order were *Compositæ*, 13 species, followed by an almost equal number (10) of leguminous plants. In the miscellaneous orders the numbers ranged from six to one species each. As the authors remark, however, these figures do "not give any idea of the degree of predominance or of the absolute quantities of any particular species, or of the number of species on any individual plot."

Further general discussions deal with the "Antagonisms between plants;" the "Organisation by means of which they maintain or improve their position, or succumb in competition with others;" "Hostile competition," and other important considerations in connection with which the valuable observations of Hoffmann, Nægeli, Sachs, Nobbe and Darwin are cited. In-

	Gramineæ and undetermined portions.	Leguminosæ.	Miscellaneous orders.	Total.
Without manure	1792·7	213·4	654·0	2660·0
400 lbs. ammonia salts alone	2652·1	8·3	496·2	3157·0
550 lbs. nitrate of soda alone	3436·5	14·1	893·1	4344·0
275 lbs. nitrate of soda alone	3184·0	33·6	929·7	4147·0
Mixed mineral manures with potash	2841·1	1010·6	702·8	4554·0
Superphosphate of lime alone	2053·1	128·6	732·3	2914·0
Mixed mineral manures without potash	2608·3	390·9	635·8	3635·0
400 lbs. ammonia salts with mixed mineral manures and potash	5191·3	10·7	672·0	5874·0
400 lbs. ammonia salts with mixed mineral manures, potash, and 2,000 lbs. cut wheat straw	5852·5	11·5	550·5	6414·0
800 lbs. ammonia salts with mixed mineral manures and potash	6268·6	0·5	329·7	6598·0
800 lbs. ammonia salts with mixed mineral manures, including potash and silicates	7041·6	0·0	216·2	7257·0
550 lbs. nitrate of soda and mixed mineral manures, including potash	5790·1	41·9	512·8	6344·0
400 lbs., ammonia salts and superphosphate of lime	3711·0	2·2	536·5	4250·0
400 lbs. ammonia salts and mixed mineral manures, with and without potash	4514·0	3·3	575·7	5093·0
Ammonia salts alone for 13 years, followed by mixed mineral manures each year since	2799·1	85·6	787·3	3672·0
275 lbs. nitrate of soda with mixed mineral manures including potash	4440·3	282·2	699·8	5422·0
Farmyard manure alone	3404·5	141·6	758·7	4304·0
Farmyard manure and ammonia salts	4225·3	26·1	673·4	4924·0

teresting facts are also developed as to the relation of special fertility to predominance of certain species as determined by their particular habit of growth; e. g. whether deep or shallow rooted, etc., as well as many other details which can only be well appreciated by a careful study of the pages.

The most valuable results obtained are those which show the influence of different fertilizers upon the character of the vegetation and the total produce, since they give us a certain clue, reciprocally, to the character and fertility of the soil as indicated by the vegetation produced thereon, considerations which are of importance at the present time in view of the efforts being made to reclaim marsh lands and other areas now of little or no agricultural value. A close inspection of the tables given will be necessary to discover the interesting relations there shown, especially where the particular species is influenced, but the general results may be shown in the preceding tabular abstract which gives the means of the results in pounds per acre, for the years from 1862 to 1877 inclusive, as obtained from the various plots under different manurial treatment.

ART. XLVII.—*Mr. Backhouse's Observations on Physiological Optics*; by W. LECONTE STEVENS.

IN the Introduction to Helmholtz's Physiological Optics, the author states that he was often obliged to desist from his experiments on account of the distressing pain caused by them. The number of persons who are willing to meet the strain implied in a continued series of experiments with their eyes is at best small; and therefore I have read with some satisfaction the criticism on my work, which has recently been made by Mr. T. W. Backhouse, of Sunderland, England.

To use the eyes satisfactorily as instruments of research requires much practice, and Mr. Backhouse's admissions show that he has been laboring under some disadvantages. My own series of papers represented a succession of steps in a prolonged investigation, during which slight modifications had to be made regarding the relative importance at first attached to some of the elements which enter into our visual judgments. Mr. Backhouse disputes the efficacy of some of my explanations, but in opposition to them I do not find in his paper the record of any experiments that can be advanced as positive evidence in favor of any other explanations. He defends Brewster's geometric theory of binocular vision, but apparently without close examination of Brewster's own presentation of this. If the theory of visual triangulation be admitted in any mathematical sense, vision by optic divergence is an impossibility, yet Mr. Backhouse admits my attainment of distinct binocular vision by optic divergence. Brewster elaborated his theory as so much pure geometry,* made no allowance for divergence of visual lines, and gave no indication that he suspected its possibility. He represented visual judgments as determined by the geometric relation between certain intersecting lines, saying "the line of visible direction does not depend on the direction of the ray, but is perpendicular to the retina,"† and that "distinct vision is obtained only on one point of the retina."‡ My experiments have proven that geometry has to be thrown out of consideration in this connection, and that the term "visual triangulation" is merely a misleading analogy. Mr. Backhouse does not deny the attainment of stereoscopy with optic divergence, but yet says "Brewster's theory is not contradicted by the experiments." Optic convergence accompanies most cases of binocular vision; but accompaniment and cause are not necessarily identical. The determination of locality by lines leaves no room for play of judgment. I have never underrated the importance of optic convergence as one of a number of elements that serve as data in the unconscious formation of visual judgments; but the intersection of imaginary lines may be safely discarded. Associated muscular action is an important element, but very far from sufficient by itself. Rapid and unconscious play of the eyes, as noted by Professor LeConte, is another important element in many cases, but by no means wholly sufficient, as is proven by my binocular experiments with spectral images. Regarding the metaphysics of corresponding retinal points discussion would probably be useless. I have repeatedly insisted upon the correspondence of certain retinal parts, but denied the strict mathematical interpretation of this given by Brewster. I can regard it only from the standpoint of evolution, as a product of experience, individual and ancestral.

Mr. Backhouse on p. 306 restates my threefold division of the elements of physiological perspective, including that of focal adjustment, and refers in a foot-note to my enumeration of the elements that are not physiological, but says that in the latter list "focal adjustment is somehow omitted." Since this element is exclusively physiological, the criticism is correct but perhaps unnecessary.

In his observations on the "phantom wall," the curvature of which was noticed but not explained by Brewster, Mr. Backhouse shows that his negative results are due to the insufficient muscular power of his eyes. All strain is removed by resorting to the device described in my paper on that subject. He is mistaken in supposing that this involves any change of the conditions necessary for producing the appearance of curvature. The effect has been attained by many persons, including a number of well-known men of science.

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* See Edinburgh Transactions, vol. xv, Part III, p. 360.

† Brewster on Optics, p. 246.

‡ Ibidem, p. 50.

In regard to the combination of points differing in retinal latitude, the only appeal is to experiment. My observations corroborate those of Professor Rogers and of Helmholtz. The nature of the experiment seems to have been misapprehended by Mr. Backhouse. It invalidates the theory of corresponding points, if any mathematical significance is to be attached to this theory.

My statements regarding stereographs of the sun and moon do not convey any exaggerated idea of the facts. By taking advantage of the sun's rotation and the moon's libration such stereographs have been obtained, and the binocular effects mentioned by me have been observed. My own examination of stereographs of the moon, and of other stereographs contrived as test objects, amply warranted me in accepting printed testimony regarding the stereograph of the sun, prepared by Mr. De la Rue. Unfortunately, the photographic negative was accidentally destroyed a few years ago, so that Mr. De la Rue was unable to furnish me a copy. My statement on this one point was therefore not based on personal inspection, but on my recognition of the entire accordance between the testimony on this point and my own oft repeated experience in such matters.

Brooklyn, Oct. 3, 1883.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Homologous Spectra.*—Hartley has continued his studies upon the spectra of allied elements, and has confirmed his supposition that elements of the same homologous series present either homologous spectra or homologous groups of lines in their spectra. He uses the word homologous here, to mean possessing an affinity depending on structure or constitution. Analogous resemblances in spectra had been observed by Mitscherlich, by Lecoq de Boisbaudran and especially by Cimician. The latter concludes that each natural group of elements has its own peculiar spectrum and certain lines and groups of lines recur with decrease or increase of wave-length for differences in the spectra of different elements of any one group. Almost every element has a number of feeble lines which bear the same relation to the chief line or lines which the over-tones in music bear to the funda-

mental note. An increase of wave-length of homologous lines of similar elements corresponds with a greater intensity of chemical kinetic energy. Hartley's photographs of the ultra-violet region completely confirm these views. Stoney had shown that the spectra of gases are to be referred to periodic motions within the individual molecules and not to the irregular journeys or encounters of the molecules with each other. The three hydrogen lines, *h*, F and C are the 32d, the 27th and the 20th harmonics of a fundamental vibration whose wave length is 0·013127714 mm. A harmonic relation exists also between the lines in the spectra of magnesium, zinc, cadmium, aluminum, and in calcium, strontium and barium when two octaves of the spectrum are examined. The fundamental vibrations appear to be in the infra-red region. In conjunction with Adeney, Hartley has accurately determined the wave-lengths of lines in twenty spectra. To study conveniently the harmonic relations between lines and groups of lines, it is necessary to map spectra according to their inverse wave-lengths or oscillation frequencies. In the spectrum of magnesium, there are three single lines, one pair of lines, four triplets, a quadruple and a quintuple group. In the zinc spectrum, a single line, a pair, three groups of triplets, a quadruple and a quintuple group; and the same for cadmium. Copper and silver each give two double groups, one sextuple and one septuple group. Aluminum three single lines alternating with three pairs. Boron two single lines and a pair. Silicon three single lines, three pairs and a septuple group. · Maps of these spectra are given plotted in oscillation frequencies. These results in the author's opinion support the view that elements, whose atomic weights differ by a constant quantity and whose chemical character is similar, are truly homologous; or in other words are the same kind of matter in different states of condensation. He regards Lockyer's hypothesis "that every elementary substance may be decomposed into as many simple substances as there are rays in its spectrum" as quite incompatible with the theory that the spectra are composed of harmonic vibrations, since a compound body would give two or more series of harmonics related to two or more fundamental vibrations; and elements having a common component should give spectra in which the same series or groups of lines appear. In illustration Hartley gives the wave-lengths of certain lines in the copper and the tellurium spectra as follows: for copper 3307·4, 3290·2, 3280·5, 3273·6 (very strong), 3264·3, 3247·3 (very strong); and for tellurium 3307·5, 3290·0, 3280·4 (very strong), 3273·8 (very strong), 3264·4, 3247·2 (very strong). Both the substances were pure. Since the correctness of the measurements cannot be relied on beyond 0·1 tenth-meter, though they are believed to be correct to 0·2, it is impossible to say absolutely whether the above lines are or are not coincident.—*J. Chem. Soc.*, xliii, 390, Sept., 1883. G. F. B.

2. *On some Reactions of Tellurium.*—It is generally stated that tellurides in solution have a reddish-violet color. DEMARÇAY

however, finds that when pure they are slightly yellowish in color, the red-violet tint being due either to poly-tellurides or to some sub-oxide. If a strongly alkaline solution of a telluride be boiled with a reducing agent, phosphorus, hypophosphite, aluminum, etc., the violet color disappears to be replaced by a weak pale-yellow. If exposed to the air, the violet color reappears. If a tellurite be used in the above experiment the red-violet color is at first developed and then destroyed. If the solution is not sufficiently alkaline, a portion of the tellurium appears to be eliminated as hydrogen telluride. The formation of the violet compound appears to precede the formation of telluride, and may be due to the formation of a sub-oxide. The author also shows that Wöhler's tellurides of methyl and ethyl may be readily obtained by heating tellurium in powder for 48 hours in contact with methyl or ethyl iodide at 80° C. The two bodies unite to form dimethyl diiodide, which decomposes slowly at 100°, rapidly at 120°. From this, other derivatives are obtained.—*Bull. Soc. Ch.*, II, xl, 99, Aug., 1883. G. F. B.

3. *On Caucasian Ozocerites.*—BEILSTEIN and WIEGAND have submitted to an examination the ozocerite obtained from the island of Tscheleken in the Caspian Sea. It was a brownish-black sticky mass, which dissolved in boiling benzene leaving but a trifling residue. On adding alcohol, most of the paraffin was precipitated, leaving the admixed oils in solution. It was found better, however, to wash the crude material with ether, to remove the oil and coloring matter; and then to extract the residue with absolute acetic ether. The nearly colorless paraffin thus obtained was dissolved in boiling benzene, treated with animal charcoal, and precipitated by alcohol. By repeating this operation, a hydrocarbon was obtained in brilliant white crystals, and of constant fusing point, evidently homogeneous. To this substance, the authors have given the name *lekene*, from the island where it occurs. It constitutes the chief constituent of the Caucasian ozocerite. It melts at 79° and has a specific gravity of 0·93917. It dissolves readily in benzene, carbon disulphide and ligroin, less so in chloroform, ether and alcohol, and is quite insoluble in absolute acetic ether. Its solutions in hot benzene, carbon disulphide and chloroform gelatinize on cooling. On analysis it gave carbon 85·23, 85·1; hydrogen 14·72, 14·57; numbers not sufficiently close to determine between the formulas C_nH_{2n} and C_nH_{2n+2}. In vacuo it distils unchanged; but at atmospheric pressure it breaks up. Lekene is very permanent. Heated on the water bath for a week with nitric acid, or treated with chromic acid at 100°, it suffers no change. With acid chameleon solution of 4 per cent strength 500 c. c. were required with one gram of lekene to produce a permanent color; thus using three parts of oxygen to one of hydrocarbon, $(CH_2)+O_3=CO_2+H_2O$, and going to confirm the formula C_nH_{2n}. Fuming sulphuric acid converts lekene into a black crumbly mass, containing no sulphonic acids. Bromine gives crystallized substitution products.—*Ber. Berl. Chem. Ges.*, xvi, 1547, July, 1883. G. F. B.

4. *On the Fermentation of Cellulose.*—**Tappeiner** has confirmed his assertion of a year ago, that cellulose is a fermentable substance, and now gives the results of his closer study of the phenomena attending it. When to a flask containing finely divided paper or cotton and a one per cent extract of meat, sterilized, a small quantity of the contents of the first stomach of a ruminant is added, a fermentation is set up by which the cellulose is brought into solution almost entirely. Upon examination it appears that two fermentations go on simultaneously, one evolving carbon dioxide and hydrogen, the other carbon dioxide and marsh gas, though both yield organic acids also. The latter goes on slowly for four weeks, evolving CH_4 and CO_2 in the ratio of 1 : 7·2, besides SH_2, H and N, at the beginning, and in the ratio 1 : 3·4 at the end. The remaining liquid, which had an acid reaction, was fractionated and yielded acetic aldehyde, acetic acid and other volatile fatty acids. The other cellulose fermentation, in which hydrogen is evolved, goes on when the neutral one per cent solution of meat extract is replaced: (1) by such a solution weakly alkaline; (2) by one diluted with an equal volume of water, containing in 100 parts HK_2PO_4 0·2 gram, $MgSO_4$ 0·04, $CaCl_2$ 0·02, called Nägeli's solution; (3) or by aqueous solutions which contain, besides the above mentioned salts, nitrogenous bodies, such as 0·35 ammonium acetate, 0·3 acetamide or 0·6 asparagin in 100 parts. With the second of the above named solutions, the gas evolved consists of 55·39 CO_2 with a small quantity of SH_2 and of 42·71 H, the difference 1·90 being nitrogen. A control-flask treated in precisely the same way, but containing no cellulose, evolved no gas. On examining the residue in the flask, the same bodies were detected as in the first fermentation. Hay, enclosed in a flask with air and water, evolves gases consisting of hydrogen and carbon dioxide and produces volatile acids. The author applies the facts he has observed to the phenomena of marsh gas formation from cellulose in ponds and marshes.—*Ber. Berl. Chem. Ges.*, xv, 1734, July, 1883. G. F. B.

5. *On the Splitting of Inactive Mandelic acid into two Isomers both optically active.*—**Lewkowitsch** has succeeded in proving conclusively that the optically inactive mandelic acid consists of equal portions of dextro- and lævo-rotatory mandelic acid, and in splitting the inactive acid so as to produce the active ones, and uniting the latter so as to produce the former again. Under the influence of fungoid ferments, this splitting takes place, one of the component acids being consumed by the plant. With *Penicillum glaucum*, the lævo-mandelic acid is destroyed, the dextro-mandelic acid remaining in solution. With *Saccharomyces ellipsoideus*, the dextro-mandelic acid is consumed and the lævo-mandelic acid is left. With cinchonine, the dextro-mandelate crystallizes out, leaving the lævo-mandelate in solution. The author proposes the name paramandelic acid, for the inactive acid thus formed.—*Ber. Berl. Chem. Ges.*, xvi, 1568, July, 1883.

G. F. B.

6. *On the Condition in which Carbon exists in Steel.*—**Abel** and **Deering** have made a series of experiments in the hope of obtaining information on the condition in which carbon exists in steel as it is left by the cold-rolling, as well as in its hardened, annealed and intermediate conditions. Two series of experiments were made. In the first series the steel was in the form of 12 disks 2·5 inches in diameter and 0·01 inch thick, each weighing about 6·5 grams. In one disk, hardened, the silicon was determined to be 0·20 per cent. A cold-rolled disk gave 1·108 total carbon, a hardened one 1·128, an annealed inside disk 0·924, and a similar outside one 0·860. Three disks were used for estimating the so-called uncombined carbon by gently heating them for three hours in 100^{cc} of hydrochloric acid of sp. gr. 1·10. The cold-rolled one left 0·096, the annealed inside one 0·052, and the hardened one 0·035 per cent carbon. Three others were placed in a cold saturated solution of potassium dichromate acidulated with $\frac{1}{10}$ of sulphuric acid, being supported on sieves of platinum gauze. The cold-rolled, the annealed and the hardened disk each left a small quantity of black particles which appeared spangly under the microscope and which were attracted by the magnet. Upon analysis the particles from the cold-rolled disk gave 1·039 per cent carbon and 5·87 per cent of iron; those from the annealed outside disk 0·830 and 4·74; and those from the hardened disk 0·178 and 0·70; calculated on the total weight of the disks. Thus the chromic acid treatment has left nearly the whole of the carbon from the cold-rolled and the annealed disks in the form of a carbon-iron compound corresponding closely to the formula Fe_6C_2. But, on the other hand, in the case of the hardened disk only about one-sixth of the total carbon was left undissolved by the chromic treatment. The last disk was submitted to the action of a chromic solution containing a large excess of sulphuric acid. The black residue gave on analysis 0·84 carbon and 1·104 iron, showing that the carbide had broken down. To study the carbon-iron compound more carefully, and especially to learn (1) whether it is independent of the strength of the chromic solution, (2) whether the percentage of carbide in the steel is constant, and (3) how much of its carbon remains unconverted into hydrocarbons upon treatment with hot hydrochloric acid, a second series of experiments was made. The steel used was in the form of a thin sheet 0·008 inches thick, weighing 175 grams, cold-rolled from an ingot melted from cemented blister steel. On analysis it contained 1·144 carbon, 0·166 silicon, and 0·104 manganese. Four experiments were made, from 7 to 7·5 grams of steel being used in each, the strength of the chromic solution varying. The results seem to show that the material separated from cold-rolled steel by the action of chromic and sulphuric acid below a certain strength contains an iron carbide approximating to the formula Fe_3C or its multiple. Hence these experiments appear to sustain the view that the carbon in this steel is not simply diffused mechanically through the mass of steel but

exists in the form of an iron carbide, a definite product capable of resisting the oxidizing effect of an agent which exerts a rapid solvent action upon the iron through which the carbide is distributed. Future experiments must decide whether the carbide varies in its composition in different descriptions of steel similarly treated, though the experiments above given suggest that hardening diminishes the power of the carbide to resist the decomposing effect of the chromic solution.—*J. Chem. Soc.*, xliii, 303, June, 1883. G. F. B.

7. *A new fluid of great specific gravity, of large index of refraction and of great dispersion.*—C. ROHRBACH describes the method of preparation of a solution of iodide of quicksilver and barium which possesses a great power of dispersion. At ordinary temperatures this solution is a clear fluid of a yellow color. The indices of refraction of various Fraunhofer lines were measured at the temperature of 23° C. by enclosing the liquid in a hollow Steinheil prism of 60° (*a*) and afterwards in a similar prism of 10° (*b*) (H_α, Na, H_β). The results are compared with those obtained from other well-known liquids in the following table:

	C	D	E	F	$n_F - n_C$	$\frac{n_F - n_C}{n_C}$
Water, 18, 75°	1·3320	1·3336	1·3357	1·3380	0·0060	0·0045
Bisulphide of carbon at 15, 65°	1·6219	1·6308	1·6439	1·6555	0·0336	0·0207
Oil of cassia	1·5979	1·6073	1·6207	1·6358	0·0379	0·0236
Faraday's heavy glass, G=4·728	1·7077	1·7148	1·7242	1·7325	0·0248	0·0145
Thoulet's iodide of quicksilver and potash, G=3·112	1·7014	1·7167	1·7391	1·7621	0·0607	0·0357
The same, G=3·2	----	1·733	----	----	----	0·0369
Iodide of quicksilver and barium G=3·564 *a*	1·7755	1·7932	1·8265	----	----	----
b	1·7752	1·7928	----	1·8448	0·0736	0·0409

The strong absorption obscures the lines beyond G in the direction of the violet. The amount of the dispersion is shown by the indices of refraction of the components of the D line $n_{D_1}=1·7931$, $n_{D_2}=1·7933$. With a prism of 60° the angular difference between these components is fully 2′.—*Ann. der Physik und Chemie* No. 9, pp. 169, 174. J. T.

8. *Assumption of a Solar Electric Potential.* — Sir WILLIAM SIEMENS, in his memoir "On the Conservation of Solar Energy," makes the hypothesis that the sun possesses a high electric potential which possibly produces the phenomenon of the zodiacal light. The existence of this electric potential is supposed to be due to the friction of the matter which on his hypothesis is drawn into the sun from space. Werner Siemens in a recent article calls attention to his observations on the electrization of the apex of the Cheops pyramid by the whirling dust clouds of the desert, as illustrating his brother's theory. "It is assumed that the body of the sun is conductive and is insulated by the sea of flame enveloping it. The photosphere retains one of the electricities

which have been separated by the friction while the other is spread by convection in cosmical space." The sun's electric potential then is supposed to act upon the earth's charge of free electricity. The dissipation of the earth's electricity, arising from the sun's distribution, is favored by the extreme rarefaction of the upper strata of air and by the ascending and descending currents of air loaded with moisture, since by these the free electricity is conveyed to the upper strata of highly rarefied air. The auroræ boreales and australes arise from the electrical compensation taking place at the boundary of the earth's atmosphere, between the matter flowing out from the sun charged with negative electricity and the liberated positive induced electricity of the earth. This exchange occurs principally in the polar regions, for the polar atmosphere is displaced by the strongly electrified air brought by the equatorial current in the upper regions of the atmosphere. Earth currents are also due to the electrical compensation which is continually going on. Mr. Siemens then refers to the investigation of Rowland upon the electrodynamic effect of a static charge of electricity when in motion and infers that the charge of the earth by its rotation is sufficient to produce the observed magnetism of the earth. He is apparently ignorant of the fact that Professor Ayrton and Professor Perry have made the same hypothesis and that Rowland in a subsequent paper has shown that the charge in the earth's atmosphere necessary to produce the observed magnetism of the earth would cause a spark to pass between the earth and the moon. Mr. Siemens regards thunder clouds as screens. As long as the particles of water constituting the cloud are at considerable distances from each other the cloud will be unaffected by the earth's electricity. Hence mist and light clouds will not become electric. But as soon as the particles of water come in contact with each other or so near to each other that electricity of high tension can leap from one drop to another the cloud is subjected to a distributive process which can be brought about or hastened by ascending vortices of clouds. By a copious formation of rain the water particles of the whirling vortices of clouds are brought closely together up to the highest air strata and it therefore becomes a conductor of electricity and is subject to electrical distribution. If at any place it is in conductive connection with the earth the electricity of the latter must flow into it. If it is not so connected it will become charged with electricity opposite to that of the earth while the similar electricity escapes by the conductive vortex cloud into the higher regions. The author of this theory suggests that astronomical investigations may show from the perturbations of the paths of Mercury, the asteroids and the satellites, the existence or non-existence of a solar electric potential.—*Phil. Mag.*, Sept. 1883, pp. 161–181. J. T.

9. *Cause of anomalous double refraction in some isometric Salts.*—The subject of the cause of the anomalous optical phenomena, observed in the case of many substances which appar-

ently crystallize in the isometric system, is one which has been widely discussed in the past few years. **Brauns** has made an important contribution to the subject by showing that in some cases the cause for this abnormal double-refraction is to be sought for in the variation in chemical composition occasioned by the presence of isomorphous compounds. For example, he finds crystals of potash alum and ammonia alum, which are chemically pure, to be absolutely isotropic, while crystals formed from a solution containing both salts showed distinct double-refraction. Similar results were obtained with crystals of lead nitrate and barium nitrate: when perfectly pure the crystals of each were isotropic, but the presence of a small amount of the isomorphous compound was accompanied by strong double-refraction. These observations supplement those of Klocke, who referred these optical phenomena to internal tension, by showing a cause why this tension should exist.—*Jahrb. Min.*, ii, 102, 1883. E. S. D.

II. GEOLOGY AND MINERALOGY.

1. *The Tortugas and Florida Reefs;* by **Alexander Agassiz.** Mem. Amer. Acad., xi, for 1882, pp. 107–134. 4to, with 12 plates of maps and sections. June, 1883.—This memoir contains the results of a careful study of the Florida reefs by Professor A. Agassiz. The author sustains the view of Captain E. B. Hunt* that the long curving Florida reef, stretching westward from the southern extremity of this peninsula, has derived its form and its length through the drifting of calcareous (chiefly coral) sands by the current in the sea. Captain Hunt appealed to the sweep of the eddy counter-current along the reef region, thus making the reef, like any sand-reef, one growing by its extremity in the direction of the prevailing current, and not dependent on any progressing subsidence for its form. Mr. Agassiz presents various facts from the Tortugas and other parts of the reef which show that this view is adequate to account for the present condition of the reef and all its various features; and the facts are finely illustrated by the maps and sections. He adds to the above cause the fact that the warm current carried abundant food for the life of the reef, and urges that this cause must have generally had great influence in determining the distribution of coral reefs. Professor Agassiz further suggests that in the case of these and other coral reefs of the West Indies the bottom may have been brought up to within 50 or 100 feet of the surface where corals could begin to grow by the accumulations of calcareous relics made through the growth of other species or through uplifts of the sea-bottom and volcanic action. He mentions the occurrence of elevated coral reefs in Cuba, San Domingo and other West India islands, made chiefly of recent corals, as evidence that such upliftings have occurred; and states also that the Shell-limestone, or "Coquina," of St. Augustine, off portions of the east shore of

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* This Journal, II, xxxv, 197, 1863.

Florida, north of Cape Florida (where no reef-making corals exist), is raised ten to twenty fathoms above the sea-level "at such points as Anastasia, Merritt's and Worth islands." The deposit is not known to be now increasing. Illustrations of the views presented in the memoir are drawn also from the Bahamas, the Yucatan reefs and from other parts of the same seas.

Professor Agassiz's observations in the West India seas lead him to question, as others have done, the view of Darwin that the atolls of the Pacific owe their forms largely to the conditions attending a progressing subsidence. It cannot be doubted that the causes acting in the American seas have acted alike in all coral reef seas; but they are adequate to produce like effects only under like conditions. The great depth of the seas of the Pacific reef region, the abrupt submarine sides of the atolls, the depth of the lagoons, in many cases, the resemblance between barrier reefs around high islands and atolls, amounting to identity of features, and the proofs of subsidence in many of the groups which the writer has elsewhere presented, make the question with regard to the oceanic atolls and reefs a very different one from that in the Florida seas. And even the sufficiency of the explanation for the Florida seas is not demonstration that subsidence has not been, to some degree, a factor in the case of other more eastern West India coral islands and in the Bermudas. When the question for the atolls has been studied by means of a series of borings to a depth of 500 or 1000 feet, which shall bring up the rock in a core six inches in diameter for examination, science will have positive conclusions as to the organic source of the reef limestone and as to the subsidence theory. Whatever the conclusions may be, geology is greatly indebted to Professor Alexander Agassiz for his careful and convincing investigations.

The writer has been misread on one point. He is made to share with Darwin in the view that Echinoderms were an important source of coral mud. But the fact is that he contests Darwin's conclusion, and, in opposition to it, makes trituration by the waves and currents the chief source of the mud as well as sand (Exped. Geol. Rep., 4to, 1849, p. 107, and Coral Islands, pp. 228, 229 and 340), saying that "fishes and Holothurians are quite inadequate for the supply," and that "the incessant grinding action about reefs accounts satisfactorily for the deposits of coral mud, however great their extent." J. D. D.

2. *Twelfth Annual Report of the U. S. Geological and Geographical Survey of the Territories of Wyoming and Idaho, for the year* 1878; F. V. HAYDEN, U. S. Geologist. Parts I and II. 504 and 810 pages, 8vo, with five large maps and numerous plates and wood-cuts.—These two volumes close the series of annual Reports of the survey carried on under the general charge of Dr. Hayden and the direction of the Secretary of the Interior—a survey that was fruitful in results to science and the country. In the letter of transmission of the Report now issued, Dr. Hayden speaks of five other volumes of the quarto series of reports as re-

maining unpublished, and adds that the general direction as to the completion and publication of these volumes has been committed to Major J. W. Powell, Director of the United States Geological Survey. This twelfth Annual Report contains Reports by C. A. White on Cretaceous, Tertiary, Laramie, Triassic and Carboniferous fossils; Report by O. St. John on the Geology of the Wind River District; Report by S. H. Scudder on The Tertiary Lake Basin at Florissant, Colorado; Report on Crustacea by A. S. Packard; Report by R. W. Shufeldt on the Osteology of *Speotyto cunicularia*, *Eremophila alpestris*, N. A. Tetraonidæ, *Lanius Ludovicianus*, and of Cathartidæ. Dr. White's Report is illustrated by 32 plates, Mr. St. John's by 49 plates; Mr. Scudder's by a map; Professor Packard's by 39 plates and a map; Mr. Shufeldt's by 24 plates; and the two last also by numerous wood-cuts in the text.

Part II is occupied with the geology and topography of the Yellowstone Park, and includes a general geological report by W. H. Holmes, and a special report on the Thermal Springs and geysers by Dr. A. C. Peale. Both are profusely illustrated and some of the plates are colored. Mr. Peale's Report has already been briefly noticed in this Journal.

3. *Report of the Second Session of the International Geological Congress at Bologna in* 1881. 664 pp. 8vo. Bologna, 1882. —A notice of the session of the Geological Congress at Bologna is given in volume xxiii (p. 150) of this Journal. The Report, issued under the supervision of the president, Professor G. Capellini, is a volume of great interest on account of the subjects discussed, and especially for its full, and in part illustrated, presentation of the reports of various national committees with reference to geological nomenclature and the colors and symbols of geological maps. The scheme of colors and symbols proposed in reports by Professor Heim, of Zürich, M. Maillard, of Paris, A. Karpensky, of St. Petersburg, and Professor Giordano, of Rome, are given in colored plates. These subjects are still under consideration, and the final decision on some points will probably not be reached before the next triennial session, that of 1884, in Berlin.

4. *Expedition to Rio Negro, Patagonia, in April, May and June*, 1879, under the orders of General D. Julio A. Roca. *Informal Official Report of the Scientific Commission.* (In Spanish). Third Part, *Geology*, by Dr. D. Adolfo Dœring. 4to. 1882. Work distributed by the National Academy of Sciences of Cordoba.—This Report treats briefly of the crystalline rocks, which are referred to the Laurentian and Huronian, though without giving any especial stratigraphical evidence as to age, and then takes up the Cenozoic formations, which are of great extent and rich in fossils. The subdivisions adopted are 1 (oldest), the Guaranitic, made to correspond to the Upper Cretaceous or Post-Cretaceous, and corresponding to the Laramie group and lower Eocene, and characterized by *Mesotherium;* the Patagonian or Oligocene, the upper containing *Megamys* and *Anoplotherium*,

lower *Ostrea Patagonica;* the Araucanian or Miocene, conning *Nesodon* and *Anchitherium;* the Pampean or Pliocene, lower containing *Typotherium*, the upper *Equus.* Above the Tehuelche or Erratic formation—the Glacial; the Postnpean or Diluvial, and the Arian formation or alluvial.

. *A Physical Survey in Georgia, Alabama and Mississippi, ng the line of the Georgia Pacific Railway*, by Messrs. CAMP-L and RUFFNER. 148 pp., 8vo, with two folded maps and tions.—This report is for the most part economical in its criptions, but gives much geological information on the coal mation, including its stratification and flexures, and illustrates geology of the country by a colored geological map.

. *Use of solutions of high specific gravity in the study of ks.*—The Sonstadt solution, of mercuric iodide in potassium ide, is now in general use by lithologists as a means of obtain- a mechanical separation of the constituents of a rock. Its , however, is somewhat limited because the maximum specific vity obtainable (about 3·17) is less than that of some common ierals. ROHRBACH has recently shown that a solution of meric iodide in barium iodide, applicable for the same purpose, be obtained whose specific gravity is as high as 3·58. This ution, although less convenient than the other because of its groscopic character, can be employed for the separation of ny varieties of garnet, amphibole, pyroxene, also epidote, axie, chrysolite, staurolite and other minerals, common in rocks, ose specific gravity is above that of the Sonstadt solution. The ical characters of this solution are described on an earlier ge (406).—*Wied. Ann.*, xx, 169, 1883.

. *Richellite: A new Mineral.*—MM. G. Cesaro and G. Despret ve given the name *richellite* to a supposed new mineral from chelle in the neighborhood of Visé, Belgium. It occurs in comct masses which have a cream-yellow color, becoming ochrelow on alteration. Luster greasy or resinous to earthy. Hards between 2 and 3; specific gravity 2. An analysis gave the lowing results:

	P_2O_5	Fe_2O_3	Al_2O_3	CaO	ign.
$(\frac{3}{2})$	28·67	28·71	1·80	5·64	35·54=100·36

100° the mineral loses 23·33 per cent H_2O, and at a red heat 0 H_2O and 6·11 HF. The authors leave the composition of mineral in doubt, but regard it as a fluo-phosphate of iron l calcium, the fluorine being, in their view, in combination with iron; the microscopic examination which the authors promise make may throw some light upon the subject.—*Ann. Soc. Belg. em.*, x, 1883.

. *Classification of Meteorites.*—Professor TSCHERMAK, who made many important contributions to the study of metetes, has been led by the results of recent microscopic study to pose some changes in the system of classification given by se in 1864. The meteorites are classified after the principles lithology according to the kind and relative amounts of their mineralogical constituents. The classification as proposed by Tschermak, with some typical examples under each head, is as follows:

I. Meteorites consisting essentially of iron: ***Meteoric iron.***

II. Meteorites having an iron ground-mass with enclosed silicates. (*a*) ***Pallasite:*** iron and olivine the chief constituents (Pallas-iron, Atacama, Bitburg); (*b*) ***Mesosiderite:*** iron with olivine and bronzite (Hainholz, Estherville); (*c*) ***Siderophyr:*** iron and bronzite (Rittersgrün, Breitenbach, Steinbach); ***Grahamite:*** iron with plagioclase, olivine, bronzite (Sierra da Chaco).

III. Meteorites consisting chiefly of olivine and bronzite with iron as a subordinate constituent; the texture mostly chondritic. ***Chondrite*** (Aigle, Knyahinya, New Concord, Pultusk).

IV. Meteorites consisting essentially of olivine, bronzite or pyroxene. (*a*) ***Chassignite:*** olivine (Chassigny); (*b*) ***Amphoterite:*** olivine and bronzite (Manbhoom); (*c*) ***Diogenite:*** bronzite or hypersthene (Ibbenbühren, Shalka); (*d*) ***Chladnite,*** enstatite (Bishopville); (*e*) ***Bustite:*** diopside and enstatite (Busti).

V. Meteorites consisting essentially of augite, bronzite, lime feldspar; with a shining crust. (*a*) ***Howardite:*** augite, bronzite, plagioclase (Frankfort, Loutolaks); (*b*) ***Eukrite:*** augite, anorthite or maskelynite (Juvinas, Jonzac, Stannern, Petersborough). —***Ber. Ak. Wien.***, July 7, 1883.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The British Association at Southport.*—The President, Professor Cayley, commenced his inaugural address by a brief mention of three distinguished members of the association who had died during the year past, Professor Henry John Stephen Smith, General Sir Edward Sabine and William Spottiswoode, and then proceeded with his subject—obligations of pure mathematics to other sciences as the sources of mathematical theories. The address of Professor O. Henrici, president of the Mathematical and Physical Section, was also mathematical, his subject being pure geometry. Mr. J. H. Gladstone, president of the Chemical Section, spoke on the nature of the so-called elements; Professor W. C. Williamson, president of the Geological Section, on the vegetation of the Coal period; Lieut.-Colonel H. H. Godwin Austen, of the Geographical Section, on the Himalayas; and Mr. Wm. Pengelly, president of the Anthropological Section, on Cavern researches. Professor Williamson remarked on the strange fact that while extremely delicate cell-structures are preserved in the Carboniferous beds, no terrestrial cellular Cryptogams have been found in them except a few imperfect Fungoid relics. In the course of his review of the relations of the genera of Coal plants, he observed that *Calamodendron* and *Calamites* are the same in type and both Equisetaceous; that one tall and slender *Calamites*, found at the Moorside Colliery, near Ashton-under-Lyne, proved on measurement by him to be 30 feet

length and 6 inches in diameter at one end and 4½ at the other; at the *Sigillariæ* and *Lepidodendra* are alike lycopodiaceous ıd Stigmaria the true root of either; that these ancient lycopo-aceous plants, and also the Calamitean, even slender kinds, de-:lop a secondary exogenous growth of vascular tissue-making ›nes and so giving strength to the exterior, and that on this ructure was based the reference of the Sigillariæ and the Cala-odendron to the Gymnosperms; that *Haloniæ* are not the roots ' Lepidodendra or other plants; that the *Trigonocarpa*, *Cardio-rpa* and *Cordaites* are Cycadean, as well shown by Grand-Eury ıd others; that exogenous plants are represented by stems or 'anches, named by him *Astromyelon* (more or less aquatic since closely resembles those of *Myriophyllum*, *Marsilea*), *Heteran-ıım Grievii*, and *Lyginodendron Oldhamium*, a plant that at-ined arborescent dimensions.

The president of the Biological Section, Professor E. Ray ınkester, spoke of the small amount of scientific research in reat Britain and of encouragement toward it by the govern-ent, compared with the same in the nations of the Continent, pecially Germany, and by way of contrast, of the condition and aracter of the systems of education in German universities. He ›sed with remarks on "a reasonable and proper method of moving, to some extent, the defect in British national life due the want of provision for scientific research."

The addresses are published in numbers 725, 726, 727 of ature, and other proceedings of the Association in following ımbers.

At the next meeting of the British Association, in Montreal, ›rd Rayleigh will be the President, and the Marquis of Lorne, arquis of Lansdowne, Sir Charles Tupper, Sir A. T. Galt, Pro-ssor Huxley, the Right Hon. Lyon Playfair, Principal Dawson ıd others, Vice-Presidents.

2. *Greenland ice.*—Nature of September 27 and 28 (Nos. 6, 727) states that the inland ice party of Baron Nordenskiöld ırted on July 3 from Auleitswikfjord. At a point eighty miles st of the glacier border and 5,000 feet above sea-level they were evented by soft snow from proceeding with sledges; and Lap-nders on snowshoes were sent on from this point, who pene-ated 130 miles eastward over unbroken snow to a height of 000 feet. Over the whole inland there is ice. With the ice :cur masses of fine dust, which is partly of cosmical origin. he party consisted, under Nordenskiöld, of Dr. Berlin, Sergeant jellström, Herr Johannesen, two hunters, Sevalsen and Kræm-er, two sailors, and the Lapps, Anders and Lars. The journey sted a month.

3. *U. S. Coast and Geodetic Survey for* 1881, J. E. HILGARD, ıperintendent.—The memoirs presented in the appendix to this .eport are the following: On type-forms of topography, Colum-ia River, by E. HERGESHEIMER; Collection of results for decli-ation, dip, and intensity, from observations by the survey be

tween 1833 and 1882, pp. 159–224, by C. A. SCHOTT; Barometric hypsometry and reduction of the barometer to sea-level, Part III, by Wm. FERREL; Report on the oyster beds of the James River, Va., and of Tangier and Pocomoke Sounds, Md. and Va., illustrated by maps and plates, by Lieut. F. WINSLOW; On the length of a nautical mile, by J. E. HILGARD; On a method of readily transferring the underground mark at a base monument, by O. H. TITTMANN; On the flexure of pendulum supports, by C. S. PEIRCE; On the deduction of the electricity of the earth from pendulum experiments; On a method of observing the coincidence of vibrations of two pendulums, by C. S. PEIRCE; On the value of gravity at Paris, by C. S. PEIRCE; New rule for tides in Delaware Bay, by H. MITCHELL.

Besides these papers the appendix contains a "*General Index* of scientific papers, methods and results contained in the appendices to the Annual Reports of the U. S. Coast and Geodetic Survey, from 1845 to 1880 inclusive," by C. H. SINCLAIR, which, on account of the great value of the papers, will call forth the hearty thanks of all interested in the progress of science.

Hydraulic Tables for the Calculation of the Discharge through Sewers, Pipes and Conduits, based on Kutter's formula, by J. P. Flynn, Civil Engineer. 136 pp. 16mo. New York, 1883 (D. Van Nostrand).

Phytogeogenesis. Die vorweltliche Entwickelung der Erdkruste und der Pflanzen in Grundzügen dargestellt von Dr. Otto Kunze. 214 pp.. 8vo. Leipzig (Paul Frohberg).

Report of the Mineral Resources of the United States for 1882 and the first six months of 1883, by Albert Williams, Jr., U. S. Geological Survey. 814 pp. 8vo, 1883.

A Summary of the Geology of the Comstock Lode and the Washoe District, by GEORGE F. BECKER. Extr. from the Ann. Rep. U. S. Geol. Survey, 1880–81. pp. 293–330, with colored maps. Washington, 1881.

Geological Survey of Illinois, A. H. Worthen, State Geologist. Vol. VII. 374 pp. 8vo, with 31 plates. May, 1883.

Lehrbuch der vergleichenden Anatomie der Wirbelthiere auf Grundlage der Entwicklungsgeschichte, by Prof. Dr. R. Widersheim. 2nd part (and last), 477–908 pp., June, 1883 (Gustav Fischer).

OBITUARY.

JAMES LAWRENCE SMITH.—Dr. J. Lawrence Smith died at his residence in Louisville, Kentucky, on the 12th of October. He was born near Charleston, South Carolina, on the 16th of December, 1818, and was therefore in his 65th year. He was a graduate of the University of Virginia and of the Medical College of Charleston. After leaving the latter institution he spent three years in Europe, and mostly at Paris, pursuing studies in medicine, physics, chemistry, geology and mineralogy. While in Paris he commenced his publications in chemistry by a paper on the means of detecting arsenic in the human body, which was republished in vols. xl and xlii of this Journal (1841, 1842). After his return to Charleston, which took place in 1844, he became Assayer of the State, studied its marls, ores and cotton-bearing soils; and his able report on cotton-growing led to his appointment under the Turkish Government with reference to instruction n the subject in that country. The emery mines of Turkey gave im a new subject for research; and his report on them is the irst full account of the geological character and mineralogy of n emery region that had appeared. Besides a careful chemical tudy of the emery, in the course of which he devised the mode f attack of this refractory material by sodium bisulphate, he investigated its effective hardness by a new method and extended his determinations to the emery of other localities. The memoir was honored by the French Academy, to which it was presented, by publication in the *Mémoires des Savants étrangers.* Dr. Smith afterward studied the emery of Chester, Massachusetts, in which he confirmed and extended his previous observations on the mineral and its mode of occurrence and mineral associates. Dr. Smith published also a chemical report on the Thermal waters of Asia Minor.

American mineralogy owes very much to Dr. Smith for his careful chemical investigations, the earlier of which were carried on conjointly with Professor Brush; and the science of Chemistry in many ways, and especially for the method proposed by him, and since adopted generally, for the determination of the alkalies in silicates. Many ingenious appliances were proposed by Dr. Smith in the department of chemistry and physics; one of the most important is that of the inverted microscope. Meteorites were with Dr. Smith a subject of great interest and of thorough study. His papers giving descriptions and the results of his chemical and physical investigations of different meteoric stones and irons are very numerous, and have advanced greatly this department of science. It was the topic of his last paper, which appeared in this Journal in June of the present year. Dr. Smith published in 1873 a volume containing a collection of his papers to that date, and out of the 400 pages, 100 are devoted to his papers on meteorites.

Dr. Smith held for some years the position of Chemistry in the University of Virginia, and, during the later years of his life, that of Chemistry in the University of Louisville. His very large collection of meteorites with that also of minerals was given by him two years since to the Polytechnic Society of Louisville. He was a member of various scientific academies, both foreign and American, and was recently honored with a foreign membership of the Academy of Sciences of Paris. One who well knew him in his adopted city says justly of him: "Eminent in his profession, he was more than eminent in his home. He was a gentleman, truly, but he was a man of affairs, a man of convictions, a man among men, who, though absorbed in scientific pursuits, took a sincere and profound interest in public questions and events. He had not an enemy on earth, despite the positivity and transparency of his opinions, and he goes to his last rest leaving the people with whom he was so long identified to mourn the loss of a citizen of whom all were proud and whom everybody loved and honored."

Wm. A. Norton.—The death of Professor Norton, of the Sheffield Scientific School in Yale College, is announced on page 332 of this volume. As the principal portion of his contributions to science have appeared in this Journal we need only give a general statement of their character and extent. His earliest memoir was in the 46th volume of the first series, and was on the mode of formation of tails of comets. The manner of action of a solar repulsion in producing the comets' tails was developed at length. Some of the ideas, though original with Professor Norton, had been anticipated by Olbers and Bessel. A series of papers followed upon the relations between the distribution of heat on the earth and the phenomena of terrestrial magnetism.

From these he was led on to further discussion of magnetic action over the earth, and of like action, as he argued, in the body of the sun and in the formation of the comas and tails of comets. These papers included especially an elaborate discussion of the famous comet of 1858.

After this followed a series of papers on molecular physics, in which, starting from a few elementary assumptions, he arranged in one system the various phenomena of physics, explaining the gaseous, liquid and solid forms of matter, the various phenomena of electricity and magnetism, of light, heat, attraction, crystallization and chemical action; also explaining terrestrial, cometary and solar physics, the whole worked out in detail. Many of his conceptions and arguments are in direct opposition to widely accepted theories. But if some, or even if all of them shall, in the end, fail to be accepted as truths of nature, yet these memoirs will continue to testify to his love of truth, his painstaking labor and his complete grasp of the problems to be solved.

Professor Norton graduated from West Point in 1831, and for the next two years held the position in the Military Academy of Professor of Natural and Experimental Philosophy. He then resigned from the army, and accepted a similar position in the University of the City of New York. He was afterward connected with Delaware College at Newark, Delaware, first as Professor and then as President, and subsequently with Brown University at Providence, Rhode Island, as Professor of Natural Philosophy and Civil Engineering. Since 1852 he has been Professor of Engineering in the Sheffield Scientific School, and since 1873 a member of the National Academy of Sciences. Commencing his work at the Sheffield Scientific School when it consisted of but two professors besides himself, he contributed largely, by his faithful labors and excellent judgment, to its development and success.

Dr. Oswald Heer, the distinguished author of works on fossil plants, and the most prominent of investigators of Arctic species, died at Lausanne on the 27th of September, aged 74 years.

Joachim Barrande, the eminent paleontologist, died in October at Prague, in his 84th year.

THE

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[THIRD SERIES.]

ART. XVLIII.—*Some points in Botanical Nomenclature; a Review of "Nouvelles Remarques sur la Nomenclature Botanique, par M. Alph. de Candolle,"* Geneva, 1883; by ASA GRAY.

SIXTEEN years have passed since M. de Candolle laid before the International Botanical Congress held at Paris, August 16–26, 1867, a body of Laws of Botanical Nomenclature, which he had drawn up for consideration by that assembly. The code was discussed by a special committee, afterward by the Congress in full session, some modifications introduced, and it was then all but unanimously voted, "by about one hundred botanists of all countries:" "That these Laws, as adopted by this Assembly, shall be recommended as the best guide for Nomenclature in the Vegetable Kingdom." The adopted Code, with an extended Commentary, was published by DeCandolle early in the autumn of the same year; and an English Translation, made by the lamented Dr. Weddell, appeared early in 1868. The "Laws," but without the more voluminous explanatory commentary, were reprinted from the English translation in this Journal in July of that year, occupying only twelve pages; and some remarks and suggestions by the present writer were appended. As was then said, the code did not make, but rather declare, the common law of botanists. It announced principles, systematically and perspicuously, and indicated their application in leading cases; but many practical questions, as well as conflicts of rules in particular instances, which would inevitably come up, were necessarily left to be settled when they arose. No small discussion upon certain details has in fact ensued, in which our author, naturally appealed to, has taken an active part.

In the first part of the present publication (of 79 pages, 8vo), the discussions of the intervening years are summed up and reviewed; and a few changes, which experience has shown the need of, are proposed. In the second part the author takes up certain questions which the Paris Congress left untouched, such as the nomenclature of organs (which he had treated in his recent *Phytographie*), the nomenclature of fossils, and the rules according to which names and authorities should be cited when old genera are combined or reconstituted; also some matters of orthography and punctuation are briefly considered. Finally, in a third part, the Laws adopted by the Paris Congress are reprinted, with the suggested changes. The alterations and additions are printed in italic type, so that they may be seen at a glance.

We are not sure of an English edition; and in any case it is desirable to make so important a publication as this generally known to our own naturalists. So our abstract and comments may run to some length. They are intended partly to illustrate and re-inforce the author's doctrine, in respect to matters upon which there is still diversity of opinion and practice, perhaps occasionally to offer a criticism or suggestion; also as helps and guides to botanists in our own country, who are beginning to take interest in such matters and to feel (or at least to show) the need of giving attention to them.

As respects all the weightier matters of the law, the most experienced phænogamous botanists are in general agreement. Having accepted the code of 1867, they will be ready to accept proposed modifications and interpretations which are in accordance with its principles. Among them all, perhaps no one is so well qualified as DeCandolle, by the bent of his mind and course of his studies, his opportunity of leisurely consideration, and his long editorial experience—combining, as it may be said to do, that of two generations,—for declaring what the present consensus of authority is, and for discussing the cases which still need adjustment, some of them of considerable consequence in phytography.

At the head of the publications upon nomenclature emanating from or sanctioned by associations or committees of naturalists, which have appeared since the year 1867, DeCandolle places the Report to the American Association for the Advancement of Science, at the Nashville meeting, 1877, on Nomenclature in Zoology and Botany, prepared by Captain Dall, after much conference with leading American naturalists. The differences between these rules and those of the botanical code nainly to questions mooted by the zoologists, whose sys- nd views had been comparatively loose and variant. st alteration in DeCandolle's revised draft is adopted all's report, with high commendation, viz: in t. 3. The essential point in all parts of nomenclature *ity in names;* 2. Avoidance or rejection of forms or which may create error or ambiguity or introduce con- into science." The clause in italics is taken from the an code, and is said to supply a real omission. Al- merely declaratory, some practical consequences flow t. The same idea dominates in the Report made by le, chairman of a committee of the Geological Congress gna in 1881, and which concerned itself with nomen- in paleontology. This report insists that "The law of being fundamental in nomenclature, it appears to be ry to apply it with all possible generality and to sup- erogations and exceptions to this law. . . . . Contradic- tween the signification of a name and the characters of s or species is no sufficient reason for changing such etc.

ther code referred to is one by a committee of the Zoo- Society of France, in 1881, M. Chaper, chairman, in this principle of fixity is said to be less prominent, xceptions being allowed.

eviewing the whole field, DeCandolle assures us that dency during the last sixteen years has been, 1, to an ing agreement of the zoologists with botanists, and 2, an ing recognition of the law of priority as the fundamental le in nomenclature, and as prevailing over considerations ance, linguistic purity, and precise meaning. But some publications of individual botanists look the other way; so two publications by Saint-Lager, *Réforme de la lature Botanique* and *Nouvelles Remarques sur la Nomen- Botanique,* published at Lyons, in the Annals of the al Society of that city;—of which it may be said, that ex effect of the multitudinous changes proposed in the making old and accepted names better, more classical, significant, has greatly strengthened the hands of those ntend for the absolute fixity and unalterableness of ed names. Whether and to what extent misspelled or ise wrongly formed names may be corrected, and whether e cases the principle of fixity should not prevail over e priority, are matters which may be discussed further e take up in the author's order the points which we specify or to comment on.

le 6 of the code declares that "scientific names should atin. When taken from another language, a Latin termination is given to them, except in some cases sanctioned by custom." Here our author asks, "but what Latin?" He concludes that the Latin of Linnæus should be the model. It is the classical language of botany, and is much more precise than the Latin of antiquity, in which very many words bear two, three, or half a dozen senses, either in the same or in different ages; while in the technical language of botany each word has but one meaning, and each idea or object is expressed by a single term. DeCandolle elaborated this point in his Phytography, to which he refers for illustrations; and he returns to it, in special applications, when commenting on art. 66, as we shall see. But, in fact, even technical language cannot always avoid ambiguities and the use of words in senses which have to be determined by the context. *Folium*, for example, in botanical description, may mean the blade of a leaf only, or this along with its petiole, or blade with petiole and stipules together, or it may even mean any homologue of the ordinary leaf.

"Art. 15. Each natural group of plants can bear in science but one valid designation, the most ancient, whether adopted or given by Linnæus, or since Linnæus,—provided it be consistent with the essential rules of nomenclature." DeCandolle now adds an article 15$^{\text{bis}}$, which is purely explanatory, but has a bearing upon subordinate questions. It is:—"The designation of any group, by one or more names, has not for its object the enunciation of its characters or history; it gives merely the name by which we are to call it." He comments upon the tendency which is often shown to mix up the question of name with quite other considerations. Before Linnæus introduced the binomial system, the names of species were at the same time names and characters. In separating these two things, Linnæus rendered a great service, and we should be careful to preserve this advantage. "A name is a name; characters are characters; the succession of names is synonymy. To mingle such different ideas leads to confusion. In these days there is a disposition to attribute too much importance to the meaning of names, and also to intermix the synonymy—i. e. the bibliographical history of the groups—with the names, at least with the indication of the author, which being commonly annexed comes to be almost a part of the same. Such complications are contrary to the general principle that different ideas should be expressed separately. If this rule is neglected, we may be led into attempts to express in the name, or with the name, the phytogenetic history of a group, that being just now one of the ideas in vogue." Names that have an appropriate meaning are very well, and botanists always endeavor to make such; but experience shows that meaningless names are in some respects better,—are generally better than names founded on ideas, which in the progress of knowledge often become false.[*]

DeCandolle's main remarks upon art. 15 relate to the point of departure for the law of priority in botany. The names of the two great classes, *Monocotyledones* and *Dicotyledones*, are of Ray in 1703, who scientifically distinguished and named them. The counterpart *Acotyledones* is of Jussieu (1789), and to all three names our author would apply the law of priority. That the name *Acotyledones* fails to express the true character is a small objection. This is only an example of the disadvantage of significant names, which may lose aptness in the advance of knowledge. Far better are such names as *Aves*, *Pisces*, *Vermes*, etc., which time and discovery can never falsify. *Cryptogamia* as the name of a class (which for meaning is hardly so good as *Acotyledones*) was introduced by Linnæus in 1735, and has maintained its place.[†] The counterpart, *Phanerogamia* (or *Phænogamia*), is not of Linnæus, but much later. Natural Families or Orders date from Jussieu's Genera Plantarum, in 1789. Cohorts and Tribes from A. P. DeCandolle's Systema, 1818. Subgenera begin with R. Brown, in 1810, according to our author. There are a few instances (without the name) in the Prodromus Fl. N. Holl. But it is in the Oudney and Denham-Clapperton paper, in 1826 (p. 16), that their use is discussed, and the mode of designating them in citation (by interpolation between generic and specific name in parenthesis) is introduced. Species (as distinguished from varieties) and the actual binomial nomenclature date from 1753, and the first edition of the Species Plantarum. There is substantial agreement among botanists as to this point of departure; and the fact that the specific phrase of earlier authors is occasionally of a single adjective does not militate against it. *Galega vulgaris*, *Lappa major* and *Trifolium agrarium* of the old herbalists were in good binomial form; but the adjectives are phrases, not specific names.

Generic names bring in a question of interpretation and usage. In his table, DeCandolle makes the point of departure for priority, Linnæus, Genera Plantarum, ed. 1, 1737. All agree, or should agree, that no anterior name has right of priority to a Linnean name or to a name adopted by Linnæus. But as respects generic names adopted by him, are we to follow Linnæus or are we not? He says, "*Tournefortius* primus characteres genericos ex lege artis condidit." And in the Genera Plantarum: "Ipsi non immerito inventionis gloriam circa genera concedere debeam,"—and so he uniformly accredits to Tournefort the generic names adopted from him; and the same as to "*Plumerius*, . . . *Vaillantius*, *Dillenius*, ... *Michelius* et pauci alii," "qui ejus vestigia presserunt." DeCandolle remarks that Tournefort had the merit which Linnæus ascribes, but that "he kept a good many adjective names for genera (*Acetosa*, *Bermudiana*, etc.)." Since Linnæus did not adopt these, they are out of the present question. Moreover, not to speak here of a score or two of really adjective generic names, Linnæus himself adopted two which Tournefort had discarded, *Mirabilis* and *Impatiens*, and deliberately made another, *Gloriosa*, in place of a proper name, *Methonica*, of a sort which, though not of the best, is now regarded as next to the best. But it is completely understood that Linnæus is not to be corrected; so *Gloriosa*, *Impatiens*, etc., remain. Are we equally to follow Linnæus in regard to names which he adopted from Tournefort and a few later authors, some of them his own contemporaries? If so, we shall continue to write *Salicornia* Tourn., *Corispermum* A. Juss., *Olea* Tourn., *Justicia* Houst., *Dianthera* Gronov., *Lycopus* Tourn., *Linnæa* Gronov. The practice of the leading botanists has been essentially uniform in this respect, from Jussieu down to DeCandolle, father and son, even to the latest volume of the Monographia, published during the current year. It seems perfectly clear, therefore—although we believe the question is not raised in this revision—that such genera are expected still to be cited as of their respective founders. And, as hardly any one doubts that Tournefortian genera suppressed by Linnæus but restored by modern botanists (such as *Fagopyrum*) are to be cited "Tourn.," it follows that only in a restricted sense do genera begin with Linnæus in the year 1737. This case, indeed, is governed by the principle in citation, so well insisted on by M. de Candolle, that no author is to be represented as saying the thing that he does not say. The alternative course is to write "*Linnæa* Linn., which is certainly what Linnæus has not said. The only authors we know of who have on principle followed this alternative—and a notable exception it is—are Bentham and Hooker in the new Genera Plantarum, against which a protest was made in this Journal when the first part was issued. They have followed the rule that botanical genera began with Linnæus so strictly as to cite even authors as recent as Gærtner for Tournefortian genera, and to ignore botanists like Gronovius, contemporary with Linnæus, and publishing since the year 1737; and it is only by an infraction of their rule that they have avoided writing *Linnæa* Linn.

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[*] Or may be essentially false from the beginning. One of our common Maples has two names, *Acer dasycarpum* and *A. eriocarpum*, both signifying that the fruit is woolly, whereas it is perfectly glabrous; only the ovaries are woolly. Yet no botanist has ever proposed to change the received name,—which is remarkable.

[†] On p. 58 DeCandolle has collected nineteen synonyms of the name *Cryptogamia*, all of later date, and specified the objections which may be brought against each of them, besides that of want of priority.

No change of rule 15 seems actually required to bring it into unison with the almost universal practice in citation. We have to understand that genera adopted by Linnæus from Tourrt, etc., and so accredited, should continue to be thus cited; the date 1737 (Linn. Genera, ed. 1), is, indeed, the point of rture from which to reckon priority, yet that botanical ra began with Tournefort; so that Tournefortian genera h are accepted date from the year 1700. That is the limit l by Linnæus, and it definitely excludes the herbalists and ancients, whose writings may be consulted for historical idation, but not as authority for names.

pon articles 21 and 22, which give rules for the names of rs and other supra-generic groups, our author offers no remarks. We venture to offer two. It being the general that *aceæ* is the proper termination for ordinal names h take their appellation from a typical genus, it is desirto conform to it as fully as well may be.* Since *Saxifrae*, *Myrsineaceæ*, *Styracaceæ*, *Gentinaceæ*, *Nyctaginaceæ*, and *Lauraceæ* and *Juglandaceæ* were adopted in the Prodroit seems to us retrograde and unadvisable to have gone in the new Genera Plantarum to *Saxifrageæ*, and the like; this upon no obvious principle, as we have *Samydaceæ*, *Core*, etc., brought into harmony with the rule. And if in the lromus we have *Styracaceæ*, why not also *Salicaceæ?* And the Genera Plantarum, *Styrax* could take the *aceæ* termin as *Styraceæ*, why not as *Styracaceæ?* If it be objected that such terminations have an unclassical aspeet, this objecapplies all the more to the cases under our second remark: iely, that too rigid adherence to the rule that names of rders, tribes, etc., shall end in *-eæ* and the like, gives us ly unpronounceable words of four or five consecutive els, or, when the diphthongs are printed in separate letters, rding to a prevalent fashion, one or two more. Of these—lipththongs written out—*Saurayjeae*, *Spiraeeae*, *Catesbaeeae*, *neeae*, *Thymeleeae*, and *Moraeeae* are the worst instances, would justify any infraction of rules.† The last and one e worst would have been avoided by writing the ordinal e *Iridaceæ*, when that of the tribe would have been *Irideæ*. ies are to be spoken as well as read, and botanists who

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rucifera, *Leguminosæ*, *Umbelliferæ*, *Compositæ*, *Labiatæ*, and the like, are ception to the rule, rightly stated, as they are not named from typical a. We shall not have any more of them, but the old ones in use are among est.

ar better to write *Spiræaceæ*, with DeCandolle. The use of this terminaor tribal names need not be objected to by those who take little pains to use orders. And those of us who are careful so to employ it, would prefer its ional use for tribes and suborders to the concatenation of vowels, which it easy to write and almost impossible to pronounce. Some quite unnecessary names in *aceæ*, such as *Vernoniaceæ* and *Eupatoriaceæ*, adopted by Delle from Lessing, are kept up, although exceptional.

have to teach think more of these things than those who only write.

At the head of his remarks upon generic names (art. 25, et seq.), our author commends to other naturalists the very clear directions given in the rules for Zoological Nomenclature, edited by Dall, for rendering Greek letters into Latin in the construction of generic and specific names. He notes, however, that the rendering of η by e is not in full accordance with Latin usage, as witness *bibliotheca*, *dialectica*, *Hecubea*, etc.

Art. 28 of the code, which is reprinted without alteration or comment, consists of a series of recommendations of points to be attended to in the construction of generic names. As such names are to be "in Latin," it would have been well to recommend that in their formation from the Greek, from which most of them are now-a-days drawn, the principles of Latin prosody should not be wholly ignored. Such names as *Trichóclădus* and *Ancistróclădus* would have been euphonious as *Trichocladia* and *Ancistrocladia*, and very little longer. *Acanthópănax*, *Didymópănax*, *Dimorphóchlămys*, *Trigonóchlămys*. *Aulacócălyx*, *Ancycócălyx*, *Pellácălyx*, *Micróchăris*, and the like, are harder to pronounce than the makers probably thought; and so of many others.

One of the actual recommendations is "Éviter les noms adjectifs." This in Weddell's version is translated, "To avoid adjective nouns:" doubtless a wrong translation. Adjective nouns we take to be substantives which are directly formed from adjectives. Not many such are likely to be made for genera; but if such good ones can be constructed as those we already have in *Nigella*, *Amarella*, *Flaveria*, *Chlora*, *Rubia*, *Leucas* and *Hyptis*, they will not be objected to. Clearly the recommendation is to avoid *adjective names* for genera. That may be done for the future, but has not been done in the past. They are contrary to the rule of Linnæus, but not to his practice. Not to refer again to *Gloriosa*, *Mirabilis*, and *Impatiens*, at least two score of obviously adjective names for genera may be counted in the first edition of the Genera Plantarum,—such as *Arenaria*, *Stellaria*, *Utricularia*, *Dentaria*, *Asperula*, *Angelica*, *Trientalis*, *Pedicularis*, *Digitalis*, and from the Greek such as *Polycarpon*. *Amphicarpum* and *Mitracarpum*, are recent names of this kind. To conform the rule to the fact it were better to state that: generic names are either substantives or adjectives which may be used as substantives, the latter, mostly feminine in gender. *Angelica* is understood to be *Planta Angelica*, *Sanguinaria*, *Planta sanguinaria*, etc.

It is recommended "To avoid making choice of names used in zoology. But it has become nearly impossible to follow this advice, nor is it now thought to be important.

Article 33 is suppressed. It was no more than a statement of the custom that personal names for species were to be nouns in the genitive (e. g. *Clusii*) when the person commemorated was a discoverer, describer, or an illustrator of the species, but were in adjective form (e. g. *Clusiana*) when the name was merely complimentary. The rule sometimes worked awkwardly; for many personal names do not take kindly to latinization in the genitive form, which are sufficiently euphonious as adjectives in *-ana* or *-anum*; and it is well to do away with a needless restriction.

Art. 34, which recorded the fact that many names of species are substantives, is the subject of a few remarks, called out by the publication of Saint-Lager, in which it is proposed to change all these into adjectives,—a proposition which botanists are not likely ever to adopt. Some of them are among the best of descriptive names, e. g. *Stellaria nemorum*, *Convolvulus sepium*, *Rhus vernix*, *Chamærops hystrix* (and such should be written without a capital initial); those which are proper names, either old names of genera or of the herbalists, are rightly said to be significant either of the absorption of a former genus, or of a transference, or as preserving a native appellation, or as indicating a likeness. "*Digitalis Sceptrum* means a *Digitalis* which had been called *Sceptrum*;" *Ardisia Pickeringia*, a species of this genus which, mistaken by Nuttall for a new one, had been named *Pickeringia*; *Rudbeckia Heliopsidis*, a *Rudbeckia facie Heliopsidis*, from its resemblance to a *Heliopsis*. Linnæus gave us many such names; and no sufficient reason appears either for discarding these, or for forbidding the discreet adoption of new ones. But we cannot commend such a name as *Senecio Bhot* for a species indigenous to Bhotan.

Art. 36 consists of a series of recommendations for the formation or adoption of specific names. Its fifth subarticle we may refer to in another connection, viz: along with Art. 48. The recommendation to "Name no species after one who has neither discovered, nor described, nor figured, nor studied it in any way," should be respected; yet there are occasions for departing from it, especially in case of new species in very large genera. Excellent and sometimes needful is the advice to "avoid names designating little known or very limited localities." We are obliged to cite—happily as a synonym—"*Helenium Seminariense*," published by a professor who thought he had discovered a new species of Helenium in the vicinity of the "seminary," in one of our Southern States, where he taught botany.

Article 40, suggested that names of varieties originated in cultivation, and still more half-breeds and sports (so important for horticulturists to distinguish), should have only fancy-names, generally vernacular, and in some form as different as possible from the Latin specific names of botany,—names which, when needful, may be appended to the botanical name of the species, when that is known, e. g. "*Pelargonium zonale*, Mrs. Pollock." This has been seconded by the editor of the Gardeners' Chronicle and other judicious experts, and is slowly making its way.

Article 42, treating of the conditions of publicity, is the subject of additional remarks. The rule is, that "Publication consists in the sale or the distribution among the public of printed matter, plates, or autographs. It consists, likewise, in the sale or distribution, among the leading public collections, of numbered specimens, accompanied by printed or autograph tickets, bearing the date of the sale or distribution." DeCandolle now remarks that distribution among the members of an exchange club, of collections not offered to the public, does not come up to the rule; also that, as Dr. J. Müller states, the distribution of specimens without characters or any indication of the reason for calling it new, is nearly tantamount to announcing a species or genus in a publication, but without characters; which Art. 46 declares is not publication. But the cases are not quite alike. The possession of the named specimen enables a botanist to ascertain its distinctions. A published description without access to specimen may or may not serve the same purpose, very often does not. Unfortunately an insufficient or even a misleading description—and we have many such to deal with—claims the same right of priority that a good one does. It is well, therefore, that publication by sufficient distribution of named specimens should be recognized. But the remark is true that, in fact, very few distributed collections fulfill all the requirements of Article 42.

Article 47, sect. 2, recommends botanists "To publish no name without clearly indicating whether it is that of an order or of a tribe, of a genus or of a section, of a species or of a variety,—in short without giving an opinion as to the nature of the group to which the name is given." Unless this is attended to, and unless citations are equally precise,—for instance, unless subgeneric names are cited as such and not as generic, and *vice versa*,—much confusion in synonymy and in indexes will ensue.

Article 48, on the citation of the authority for generic and specific names, and matters therewith connected, involves questions which have been more disputed than any other. In the revised article the phrase printed in italic type is interpolated: "For the indication of the name or names of any group to be accurate and complete, *and for the ready verifica-*

tion of the date, it is necessary to quote the author who first published the name or combination of names."* The statement might be simplified by omitting "or names" and "or combination of names," on the ground that the name of a plant is one, that it has a name, not names, its *name* being the generic followed by the specific appellation. *Ranunculus bulbosus* is one name of two words. Our veteran botanist, Bentham, has insisted upon this; and it has a bearing upon the mooted question of mode of citation of authority.

The governing principle for citation of authorship, etc., is well declared by DeCandolle: "Never make an author say that which he does not say." It is difficult to go wrong when this principle is kept in mind, and when it is also understood that the appended name of an author, or its abbreviation, makes no part of the name of the plant, but is only the initial portion of its bibliography. Those who take a different view seem to have fallen into it by failing to distinguish strictly between name and history, and especially by mixing the history of a preceding with the statement of an actual name. A single example may illustrate this. When we write "*Mathiola tristis* Brown," we give the name of a certain kind of Stock and the original authority for it; and we may, when needful, complete the citation by adding the name of the book, with the volume and page, where it was first published. If, with some, we write "*Mathiola tristis* Linn.," we make an untrue statement. Linnæus had a wholly different genus *Mathiola*, and no *M. tristis*. If we add "sp." and somewhere explain its import to be that the latter half of the name was given by Linnæus, the other half remains unaccounted for. And we have still to seek in the synonymy for the name of the genus under which Linnæus knew the plant, and also for that of the author who transferred it to *Mathiola*. If, with others, we write "*Mathiola tristis* Linn. (*Cheiranthus*)," or "*Mathiola tristis* Linn. (sub *Cheirantho*)," our longer phrase still wants the essential part of the citation. If, to secure this, we write "*Mathiola tristis* Linn. (*Cheiranthus* Brown)," our name, if it may be so called, now extended to five words and two signs in print, or of seven words when spoken, is still ambiguous and confused. It is a jumble of names, synonym, and authorities, which become explicit and clear only when we translate it into "*Mathiola tristis* Brown (*Cheiranthus tristis* Linn.)," that is, into name and synonym, with respective authorities. This is clear and literally truthful; the injection of the synonymy into the name is neither. Linnæus reformed nomenclature by freeing the name from the descriptive phrase. The school in question would deform it by rebuilding, in another way (as DeCandolle observes), ante-Linnæan phrases, only making them historical instead of descriptive.

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* M. de Candolle appears to insist upon this verification (or incipient verification) for higher groups as well as for genera and species, and would deprecate the not unusual custom in compendious floras, catalogues, etc., of omitting to cite the authority for orders, suborders, tribes, etc. We should agree with him if the omission was held to signify that the names of the groups in question, when thus simply given, were proposed as new. But in fact, authority is omitted, not because the groups are new, but because they are old and entirely familiar. Nobody will ever suppose that *Ranunculaceæ*, *Clematideæ*, etc., nakedly written, are novelties. In this regard, the nature and plan of the publication are to be considered; what is necessary in a Systema or a Genera Plantarum may be superfluous in a local or a compendious work. Indeed, it may be nearly impossible to assign the authority for the name of an order correctly, without explanation and extended references. One would wish to write, succinctly, *Cornaceæ* DC., as DeCandolle founded the order, but it was in the form *Corneæ*. A well-known order was instituted as *Onagræ* Juss., Gen. Pl., which the founder altered to *Onagrariæ;* for reasons referred to in this article we may wish to adopt the form *Onagraceæ*. As all this is most familiar matter, yet may not be correctly stated without somewhat detailed exposition, why not in a local or condensed botanical book write simply *Onagraceæ?* The proper exposition is in place in a Genera Plantarum; and it would have been better if Bentham and Hooker had critically attended to this, instead of referring merely to the preceding work of Endlicher. It would have added somewhat, yet not very much, to their great labor.

The practice of appending the authority to the name whenever a species is mentioned has been so strictly and pedantically adhered to, that many take the former to be a part of the name. To obviate this impression, it might be well to treat the names of common plants as we do those of genera; that is, to omit the reference to authorship in cases where there is no particular need for it. Not, however, so as to cause any confusion with the cases referred to in the following paragraph:

"When a botanist proposes a new name . . . . it is impossible for him to cite an author; consequently the absence of such citation suffices to show that the name of the species or other group is new. Linnæus, Lamarck, DeCandolle, R. Brown, Martius, etc., followed this course. It is then a useless complication of many modern naturalists to append *mihi*, *nobis*, *sp. nov.*, *gen. nov.*, etc., to a new name. A large majority of species, genera, and families were published without these wholly personal indications." This is good as a general rule; but the *gen. nov.* and an indication of the order or tribe are often needful.

No new comments are made upon article 49, probably because the practice of botanists generally is conformed to it. The article reads, "An alteration of the constituent characters, or of the circumscription of a group, does not warrant the quotation of another author than the one that first published the name. . . . . When the alteration is considerable, the words *mutatis char.*, or *pro parte*, or *excl. syn.*, *excl. sp.*, etc., are added," etc. The translation would have been better worded "does not warrant the quotation of another author *in place of* the one that first published the name." For, in fact, the addition of the reforming author's name to the citation is often warranted and helpful, sometimes is almost a necessity, in the case of genera. It appears that R. Brown began, in an oblique way, the practice objected to, and for which there is often a plausible excuse; and the elder DeCandolle sometimes followed it. It was only when the practice was systematically carried out by one or two authors, that the consequences became apparent—for few genera or species have now their Linnæan limits or signification—and the new rule was practically proved to be a necessity.

Among the recommendations contained in Article 36 was the ollowing. "Readily adopt unpublished names found in traveler's notes or in herbaria, unless they be more or less defective." Guided by the practice of the elder DeCandolle and his contemporaries, it used to be thought a duty, or at the east a part of common courtesy to do this, in all cases in which the author's approval could fairly be supposed. But certain inconveniences and misunderstandings have resulted, especially as to mode of citation, which have suggested its withdrawal or modification. In this Revision, M. deCandolle only adds the restriction, "or unless the author has not in advance approved the publication." This does not alter the case, except for living authors: their approval ought to be obtained or counted on: and in respect to authors no longer living a botanist takes up only such names as in his opinion ought to be published, and which he supposes the posthumous author would have approved. On the whole, it were probably better not to take up names left unpublished by a deceased botanist; and DeCandolle assigns good reasons for letting them alone. If he had modified the article decidedly in this sense he would have more fully expressed his own view, and probably have been sustained by prevalent opinion. Moreover, he might have distinguished this practice from an essentially different one, viz:—

The case of plants sent under manuscript names by a discoverer or an investigator to some botanist engaged in publication, and with a view to their publication;—of which the sending by Nuttall of new *Umbelliferæ* to the elder DeCandolle when elaborating that order for the Prodromus, is a marked and not unusual instance. For this is a practice that need not be discouraged. Any small inconvenience that may arise as to mode of citation is counterbalanced by the greater concentration in publication, new genera and species thus appearing in monographs, floras, or in the papers of leading botanists, which otherwise would have dimly seen the light in obscure or local periodicals. And they are more likely to have proper characters assigned to them, instead of vague descriptions, by incompetent or unpracticed hands, such as often try a botanist's patience.

Article 50 treats of the mode of dealing with such names as the above-mentioned after they have been published, i. e. "names published from a private document, an herbarium, a non-distributed collection, etc." It declares that such names "are individualized (Fr. *precisés*) by the addition of the name of the author who publishes them, notwithstanding the contrary indication that he may have given." This is found to mean that, although the elder DeCandolle gives us "*Eulophus* Nutt.," as the name of a genus communicated by Nuttall, with a specimen, for the purpose of its being so published in the fourth volume of the Prodromus, yet subsequent writers, looking only to the work it was published in, are to cite it as *Eulophus* DC. And that the genus which Linnæus published as "Linnæa, authore Clariss. Dr. Gronovio," we are to cite as *Linnœa* Linn. This is not only quite contrary to the regular practice of botanists from Linnæus down to DeCandolle and later, but is also contrary to the golden rule of citation, already referred to, never to make an author say something different from or opposed to that which he does say.

Appreciating this, the author of the code has now recast Article 60, so as to read, "When an inedited name has been published [by another botanist], in attributing it to its author, those who afterwards mention it ought to add the name of the person who published it; for example, *Leptocaulis* Nutt. in DC.; *Oxalis lineata* Gillies in Hook."

This is reasonable, and in the first instance such names will almost of necessity be so cited, must always be so cited when work, volume, etc., are specified. But, DeCandolle remarks that the addition will soon vanish, for instance, that the "*Cynoglosum ciliatum* Douglas, Mss.," published by Lehmann in Pugillus, etc., and in Hooker's Flora Boreali Americana, will soon come to be quoted simply as "*Cynoglossum ciliatum* Dougl.," that is, just as other names are quoted. And why not? Because, it is said, the name dating only from the publication, it is necessary to know when and where this vicarious publication was effected. For this "Nutt. in DC." may fairly serve, nearly all names published by DeCandolle being contained in the Prodromus. Not so, however, with "Gillies in Hook." Sir Wm. Hooker published very widely, in periodicals, in the Botanical Magazine, and in numerous independent works. In such cases the double citation gives little help. The experienced botanist may know where to look; the inexperienced must turn to indexes at once; for both these must be the final and the usual resort; and in them the double has little if any advantage over the single citation. Moreover, if this principle is fully applied, the number of double-cited names may be inconveniently numerous. The first volume of Torrey and Gray's Flora of North America abounds in species and genera published by them for Nuttall. If these have all to be permanently quoted "Nutt. in Torr. & Gray," why not also the many species published, say by Bentham in DeCandolle's Prodromus, in the Flora Brasiliensis, etc., and even the species published by Brown in the second edition of the Hortus Kewensis, and elsewhere? On the whole it seems probable that these double citations will be used only in first or in early quotations, or in special instances; that it will not be deemed necessary to retain them when the names become settled in Floras or general works, except in the bibliography or full reference; when, of course the "*Leptocaulis inermis* Nutt., in DC. Coll. Mem. v. 39, x. 10, et Prodr. v. 107," will fully appear. But so long as the abbreviated citation of author and publisher together is requisite, the mode of citation recommended by DeCandolle is the one to be employed.

A quite different case is that of citing, as authority for a genus or species, the name of a botanist which is not upon the record. There is reason to believe that L. C. Richard edited the Flora Boreali-Americana of Michaux, and drew up the excellent generic and specific characters of the new plants in it. There is equal reason to believe that he purposely withheld his name. Upon no just principle of citation, therefore, can the name of Richard be quoted, as the younger Richard and Kunth essayed to do. The same holds for the work of Solander in the first edition of Aiton's Hortus Kewensis. And if it does not hold for the contributions of Brown to the second edition, it is because he claimed them in his lifetime, rather than because they have been collected and republished under his name since his death. Only confusion will come from the admission of hypothetical constructive authorship. The old rule that, what does not appear is no better than non-existent, must apply to all such cases.

In the comments upon article 52, the duty of abbreviating authors' names in the normal way is insisted on, and the bad practice of doing so by leaving out the vowels is deprecated. *Michx.* for Michaux, which is partially shortened in this way, was a necessity on account of the ancient botanist Micheli. But *Crn.* for *Crouan* is intolerable. Such a name need not be abbreviated at all. Monosyllabic names should rarely if ever be curtailed. *R. Br.* has so long been used for Robert Brown that it may continue to be used, although *Brown* is better. In the other form, it may be counted among the few cases in which initial letters are used instead of the first syllable and first consonant of the second,—cases which should probably be restricted to the *L.* for Linnæus, *DC.* for DeCandolle, *H.B.K.* for Humboldt, Bonpland, and Kunth. We are not sure that DeCandolle would favor the latter.

A series of remarks is made upon Articles 29–66, taken together,—relating to names which are to be rejected or modified, and those which are to be maintained notwithstanding certain faults. As already mentioned, the tendency among working naturalists is to preserve names in spite of faults; while a few linguistic reformers, such especially as M. Saint-Lager, propose changes which would affect 733 recognized names of species in Europe alone, and ten or twelve thousand in the vegetable kingdom at large, and "this after all the endeavors of botanists for the past half century to establish the law of priority and to have more stability of names. *Sagittaria sagittifolia* and *Psamma arenaria* must be changed, forsooth, because they are pleonasms; all substantive specific names, because a great majority of specific names are adjectives, and many others because they are not sufficiently classical. For instance, *Dianthus* he would change to *Diosanthus*, *Mentha* to *Minthe*, *Hydrocotyle brevipes* to *H. brevipedata*, *Cactus* to *Cactos*, *Arum* to *Aron*, and so on. DeCandolle adds, that Cicero was not so particular in Latinizing Greek words, as witness *barbarus*, *machina*, *emporium*. As to *Pirus*, DeCandolle insists that even if *Pyrus* is not Latin it is the botanical name of the genus as adopted by Linnæus, [from Tournefort and from all the herbalists, and it is old enough to be entered as an alternative form in the dictionaries], and so is to be preserved under the law of priority. There is little danger that the reform of Saint-Lager will prevail. There is some danger that the reaction will so stiffen the rule of priority as to forbid the correction of obvious mistakes. See, for instance, the form in which Article 60 is now recast by DeCandolle: "A generic name should subsist just as it was made, although a purely typographical error may be corrected. The termination of a Latin specific name may be changed to bring it into accordance with its generic name." From this it would seem that a slip of the pen and a mistaken orthography of a man's name may not be corrected. We trust that, when the change would not sensibly affect the place of a name in an index, such obvious corrections as of *Wisteria* to *Wistaria* may prevail. We may assume that the error was typographical; for Dr. Wistar was at the time too well known in Philadelphia for Nuttall to have been ignorant of the orthography of the name. The correction of *Balduina* into *Baldwinia* brings it into accordance with the rule that personal names used for genera should be written as near as may be with the original orthography of the person's name. "*Astragalus aboriginorum*" is neither a typographical nor a clerical error. It is a hard rule that forbids us to write "*aboriginum*," still retaining Richardson's name as authority.

Botanists may take more kindly to the rule when applied to such names as *Eleocharis* and *Aplopappus*, in the formation of which the Greek aspirate was neglected. We cannot well suppose this to have been a typographical or a clerical oversight on the part of Robert Brown or of Cassini. Perhaps a majority of botanical authors have preserved the original orthography, on the ground that the right of priority, like that of a certain king, is *super grammaticum*,—while the remainder have written *Heleocharis* and *Haplopappus ;* whence some confusion in the indexes. The requirement to preserve the original form of generic and specific names and to abide by the Latin of Linnæus and his contemporaries, notwithstanding classical faults, enables us to retain such familiar names as *Ranunculus acris*, *Lathyrus palustris* and *sylvestris* (instead of *R. acer*, *L. paluster*, and *silvestris*), and to keep up *lævis* for smooth,—probably to the disgust of classical scholars.

M. DeCandolle has a note on *Diclytra* of Borckhausen, changed into *Dielytra* to make it conformable to a conjectured meaning, and then into *Dicentra* that it might agree with the etymology given by Borckhausen himself: he gives it as a case in which an excess of erudition has loaded the genus with three names in place of one; and he concludes, as do we, that it were better to have kept the original orthography, and have treated it as a name which had, through some mistake, failed of meaning. But the name having been changed into *Dicentra*, on the ground that the right word *κεντρον*, and not the impossible word *κλυτρον*, must have been intended by Borckhausen, we think it should now be maintained, although it might have been left in the original form. Moreover, the doctrine that names must not be mended and that sense is unimportant, however good and needful, is so recent that it must not be too rigidly applied to long-standing cases.

This consideration should not be wholly overlooked in the case of old and long-established genera, especially those of numerous species, for which some obscure older name has come to light. Since it is impossible to make rules for the infraction of a rule, such cases must be left to sound discretion. In our opinion such discretion would forbid the transference of the name *Stylidium* from Swartz's genus to *Marlea*, and the revival of Labillardiére's transient first *Candollea* for Swartz's *Stylidium*.

The fourth section of article 60, which enjoined the rejection "of names formed by the combination of two languages," is now suppressed. Nothing is put in its place; but let us hope that we shall not be driven to the acceptance of the specific name "*acuticarpum*" which one of our fellow-botanists has recently perpetrated. Although hybrid names are to be avoided, yet, as DeCandolle remarks, they cannot consistently be outlawed by people who accept *centimetre*, *decimetre*, *beaurocracy*, *terminology*, and the like, nor by botanists who raise no objection to *ranunculoides*, *scirpoides*, *linnæoides*, *bauhinioides*, etc.

Names of identical meaning but of different orthography, as our author insists, may well enough co-exist. In a vast genus it might be neither inconvenient nor harmful to maintain species named respectively *fluviorum*, *fluvialis*, and *fluviatilis*, at least if they belonged to different parts of the world.

We pass to some brief annotations upon the second part of the publication before us, which deals with questions not taken up by the Congress of 1867.

The first topic is that of the nomenclature of organs, which was treated with some fulness in the *Phytographie.* The remark is here repeated that the greater part of the so-called names of organs are only *terms*, that is, names indicative of the condition of organs or parts of the plant. For some of these substantive names are necessary or highly convenient, yet most were better provided with adjective terms only, which belong to terminology, not to nomenclature. Doubtless principles of fixity and the rule of priority should apply to these, both to names and to terms. But it seems unlikely that the phytotomists will at present heed the counsels of the phytographers in this matter. Yet the latter may insist that established names used in descriptive botany shall not be displaced on the pretence of getting more appropriate ones. For instance, the long-recognized name *testa* for the outer seed-coat is to be discarded because, forsooth, this covering is not always or even not generally a shell, or of the texture of earthen ware. As well ask the French to discard the word *tête* (or *teste*], because the human head, or the skull which gave the name, does not really resemble a brick or an earthen pot.

The second is upon the nomenclature of fossils. And the rule is that they are named according to laws which apply to living plants. The Bologna congress of palæontologists ordained that, to secure priority for specific names of fossils, they should be not only described but figured. DeCandolle, after consultation with Heer (whose recent death we have to deplore), concludes that this rule is too absolute. It seems to us that so long as a large part of the names of fossil plants are merely tentative and provisional, we should be content with a general approximation to the received rules in botany.

The nomenclature of groups inferior to species (varieties, sub-varieties, variations and sub-variations) is considered; but no new rules are proposed; nor is the question of sub-species discussed.

Although it is not exactly a matter of nomenclature, we should have liked that our author had considered the two modes of disposing of varieties, and had expressed an opinion as to whether the character of the species should or should not completely cover the variety or varieties assigned to it. In the former case there is a var. α, followed by β, etc. In the latter, the species is defined upon its type, without any special regard to the appended variety or varieties, which are then characterized as to the points in which they differ from the type. We prefer the latter method, as being on the whole clearer, and as a saving in names; avoiding the awkwardness or the superfluity of a varietal name for the type of the species.

Some noteworthy observations are introduced in respect to the plight which systematic botany is threatened with by what DeCandolle would call micromorphic botanists, like Jordan and Gandoger, who abandon the Linnæan idea of species altogether, and give this name and rank to what ordinary botanists take for sub-varieties. For example, we are informed that M. Gandoger divides the Roses of Europe and N. America into 4600 species, or groups provided with names similar to those of species, under numerous subgenera, which in effect take the place of genera. *Mentha* has already undergone a similar micro-metamorphosis. If this goes on, and the names should be written every one, I suppose that even the world itself could not contain the books (of indexes) that should be written. The obvious and only remedy is to relegate this kind of botany to a world of its own, with which the legitimate science need have nothing to do.

Questions having been raised as to the proper use of capital initials in certain specific names, M. DeCandolle has devoted two or three pages to this and related topics. Linnæus used capital initials only for substantive names; Lamarck employed them for personal and some geographical names, seemingly without system. A. P. DeCandolle used the initial capital systematically for all three, and even for *Alpina* when used to designate a plant of the Alps. His example has generally been followed until recently; and this is in accordance with the custom of the English language. To the objection that it is contrary to the customs of the Latin language, our author replies at some length, substantially as follows. He finds that in the matter of orthography, etc., classical writers distinguish nine phases or periods of the Latin language, of which the most classical is the seventh period, that of Augustus; that there is no foundation in classical Latin for either punctuation (the points distinguishing words, not phrases), or accentuation by signs, and that the distinction between capitals and small letters was made since the dark ages, by scholars whom a purist of our day might tax with ignorance of the proper way of writing Latin; that the object and result of all these and other innovations was greater clearness and precision; that the question is not at all one of ancient latinity, but of modern usages, both of the philologists and the naturalists; and that these have happily modified classical Latin into a medium of greater precision and clearness and better adapted to the needs of science.

Finally, we have a brief discussion of the question: "When an author has comprehended one genus in another without naming the species, can he be cited for the names of the species which implicitly result from this union?" The answer he decisively gives is: "This would be neither right, nor possible, nor practically convenient. To be correct, one should attribute to an author exactly what he has published. When it is said that the genus B should be united with the genus A, this is not saying that the species of the genus B should be called by such and such names in the genus A. To name them correctly it is necessary to examine them one by one. A glance at the Genera of Bentham and Hooker, or at the works of Baillon, will show how impossible it is to attribute the designation of the species to the authors who have changed the names of the genera, without an explanation under each species." This is illustrated by the supposed case of three genera, combined into one, each of which has a species *lanceolata;* by the case of a species *minor* transferred to a genus of which it may be the largest species; and by reference to the state of all large and many small genera, full of obscure, misunderstood, or debatable species, the arrangement and naming of which can be effected only by patient and prolonged study. When this work has not actually been done by the reformers of genera, it should be left to monographers and the editors of Floras. If, by art. 45, "a species is not looked upon as named unless it has a generic name as well as a specific one," neither is it named unless a specific as well as a generic name is assigned to it. Besides the instances in which the old specific name is impossible under another genus, there are very many in which it would be improper or questionable, and in respect to which particular consideration is required. Between such cases and the plain ones in which implied naming could not go wrong, who is to draw the line? Perhaps it might be drawn at monotypic new genera with old specific names. But how to do even this upon recognized principles is a problem.

A fatal objection to the principle of names by implication is that all such names, if they are existent, must be indexed in the new Nomenclator Botanicus now in preparation. To transcribe under *Senecio* the specific names pertaining to all the genera which Bentham has referred to that already vast genus is no small matter, and a part of the work will prove superfluous if

—as we suppose to be the case—some of these genera, such as *Cacalia*, ought to be maintained. But that is only the beginning. A more recent author, Baillon, has reduced the genera of *Compositæ* nearly one half. For example, to *Helenium* he has referred *Gaillardia*, *Actinella*, *Cephalophora*, etc.; to *Tagetes* he has referred *Dysodia*, *Nicolettia*, *Hymenatherum* and others; to *Helianthus* a greater number of genera, most of them prolific in species. In all probability, most of these reductions will not be approved. Yet, if the principle of constructive naming is adopted, the Nomenclator must burden its columns with these hosts of inchoate specific names of Baillon, either as received names or as synonyms. It is plain that the principle referred to, besides its incongruity with the leading ideas of received nomenclature, breaks down with its own weight. There are, nevertheless, taking arguments in its favor, which need not here be recapitulated; and the common system has its disadvantages and liability to abuse. Yet it appears to be the only workable system. As already intimated, the right assignment of specific names in reconstructed genera requires particular knowledge and careful investigation. And the botanist who reconstructs genera should himself adjust and state the specific names as far as he can.

ART. XLIX.—*Pre-Carboniferous Strata in the Grand Cañon of the Colorado, Arizona;* by CHARLES D. WALCOTT, of the U. S. Geological Survey.

DURING the month of November, 1882, the Director of the Survey had constructed, under his immediate supervision, a horse trail from the brink of a lateral cañon on the east face of the Kaibab Plateau, Arizona, down to the more level cañon bed of Nun-ko-weap Valley 3,000 feet below. Encamped in the snow, often concealed for days in the driving frozen mist and whirling snow, the party gradually overcame the apparently insurmountable obstacles in the way, and, Nov. 24th, camp was formed in the supposed "inaccessible" depths of the head of the Grand Cañon, a day of reconnoissance and rest. Then the director headed his party of faithful, energetic men and left the writer, who, through illness, had been unable to share in the building of the trail, with three men and outfit to explore and study the inner cañon valleys between the Kaibab Plateau and the Marble Cañon, and the Grand Cañon as far as could be reached south. Seventy-two days of constant work gave some of the information wished, a portion of which is used in this article.

One of the strongest topographical features of the Kaibab division of the Grand Cañon of the Colorado and the lateral cañons opening into it is formed by the basal member of the Carboniferous group, the massive Red Wall limestone of Gilbert. Breaking abruptly off at the foot of the terraced slope of the Lower Aubrey sandstone, a sheer cliff of from 800 feet at the head of the Grand Cañon, to 1,000 feet at the mouth of the Kanab Cañon, is entirely formed of this limestone. Frequently several hundred feet are added to the cliff by the abruptly breaking away of the thin Devonian series and the massive Upper Tonto (Cambrian) beds beneath.

In the field work of 1879 a line of demarcation was established at the base of the Red Wall limestone, and the presence of the Devonian between it and the Tonto group of Gilbert definitely determined on both stratigraphic and paleontologic evidence. A plane of unconformity by erosion, not dip, was found between the Carboniferous and Devonian, and also a strongly marked fauna compared with that of the Tonto beneath and Carboniferous above.*

This horizon was traced, during the winter of 1882–3, from the most northern exposure in Marble Cañon, where the base of the Red Wall limestone first rises above the river-bed, to a point in the Grand Cañon south of Vishnu's Temple. From the known structure of the cañon walls, this line is undoubtedly present the entire length of the cañon to its termination at the Grand Wash. It is known in and at the mouth of the Kanab Cañon, and Mr. Gilbert's section indicates its presence at the mouth of the Grand Cañon.

In places the Devonian is entirely absent, either through erosion or non-deposition, so that the Red Wall limestone rests directly on the massive calciferous strata of the Upper Tonto. The line of unconformity is slight and often none exists except to the eye of the geologist looking at that exact horizon for it.

The Devonian rarely has a thickness of more than 100 feet. When present it is unmistakably marked by the thin purplish colored layers of fine-grained sandstone that pass into calciferous sandrock and limestone in which Cyathophylloid corals, casts of Brachiopods and Gasteropods, and plates of Placoganoid fishes usually occur.

In the Kanab Cañon a strong line of erosion was observed at the base of the Devonian. Usually this is very slight and resembles the plane of erosion at the close of the Paleozoic. In the one case the hard massive limestones of the Cambrian were eroded and in others the friable, sandy shales of the Permian.

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* This Journal, III, 1880, vol. xx, p. 221.

No traces of the Silurian groups have been discovered. At the head of Nun-ko-weap valley, where the Devonian is absent, species of the genera *Lingulepis* and *Crepicephalus* were found within two feet of the base of the Red Wall limestone. On examining the few imperfect fossils, collected in 1879 in the Kanab cañon, on which the strata just beneath the Devonian were referred provisionally to the Calciferous horizon, they were found to be generically identical with species from strata lower down in the Tonto that are the geologic equivalents of the Potsdam sandstone of the Mississippi valley.

The Tonto is the first of Powell's great plateau system of formations. Of Upper Cambrian age, it terminates below that great series of comformable deposits that extend upward through over 14,000 feet of strata to the Tertiary Pink Cliffs of Southern Utah. It occupies an important position in the topography of the inner portion of the Kaibab division of the cañon. By the breaking away of the central belt of shaly calcareous and sandy strata, the Red Wall cliff is kept clear of debris and undermined so as to preserve its mural face above a gently sloping terrace that extends down to the lower Tonto cliff. The latter is wonderfully persistent for a cliff not over 300 feet in height. Winding in and out, here a bold headland, there notched by a narrow, profound cañon, a mural precipice in range for a mile or more, then forming a symmetrically curved point around which the contours sweep in graceful curves only to recur again at the head of a broad, open cañon, the cliff is only second to the Red Wall in its influence on the scenery and topography of the inner portion of the cañon.

The geologic age of the Tonto group is determined by the presence of numerous specimens representing the genera *Cruziana*, *Lingulepis*, *Iphidea*, *Conocephalites*, *Crepicephalus* and *Dicellocephalus* that occur at various horizons in the upper 700 feet of sandstone, shales and limestone. No fossils were obtained from the coarse sandstone forming the lower 300 feet. The fauna above, however, shows close relations to that of the Potsdam sandstone horizon of Central Nevada, the Mississippi valley and Saratoga county,* New York.

The base line of the Tonto is quite uniform and rests unconformably on the varied strata beneath ; here and there a knoll, point or ridge is seen on the Pre-Tonto surface that rises nearly through the massive Tonto sandstones that were deposited against and over them, the sea breaking off, and burying with the drifting sand, fragments of the rocky islands.

* The Potsdam fauna of Saratoga county occurs in a massive bedded magnesian limestone and will be described later. The genera *Ptychaspis*, *Crepicephalus*, *Dicellocephalus*, etc., are well represented.

The great unconformity beneath the Tonto has been described by Professor J. W. Powell, who examined it in his boat trips down through the Grand Cañon, and by Captain C. E. Dutton, who viewed it from the summit of the Kaibab Plateau, five miles away. Professor Powell estimated the strata beneath the Tonto and above the Archean at 10,000 feet, and as this is all cut across the unconformity is very great. A detailed study adds to the thickness of the strata; it shows that the original summit of the Pre-Tonto group had undoubtedly been cut away more or less before the deposition of the Tonto group; that the plane of erosion cut deeply into the Archean, and that besides the 13,000 feet of strata, that have been planed off, of which the record is found in the section preserved, there was also a problematical amount of considerable thickness.

Professor Powell called the Pre-Tonto group the Grand Cañon group. It is found convenient on stratigraphic grounds to divide this into two groups, a line of unconformity by erosion occurring between them and also a decided change in the character of the sediments. The lower, the Grand Cañon group, is made up of an immense mass of sandstones and interbedded greenstones exposed directly on the Grand Cañon, and the upper, the Chuar group (a name given by Professor Powell), is a series of sandy and clay shales in the interbedded sandstones and limestones that are exposed in the inner cañon valleys between the Kaibab Plateau and the six great buttes forming the west side of the lower portion of Marble Cañon.

The summit of the Chuar group is, as now known, in a little synclinal on the divide between Nun-ko-weap and Kwa-gunt valleys.*

At first a rough sandstone, it gives way below to sandy and thin argillaceous shales with interbedded sandstones and limestones, 285 feet of limestone occurring in 5,170 feet of sediments.

The summit of the Grand Cañon group is a massive magnesian limestone, 100 feet in thickness, that overlies red sandstones resting on a massive belt of greenstones 1,000 to 1,300 feet in thickness; this belt is broken up into eight principal flows by partings of sandstone deposited between the flows. The first coulée flowed over the level ocean bed in which 5,000 feet of sediment that now forms a reddish-brown sandstone had accumulated on the upturned and eroded edges of the Archean, the few layers of limestone and the one flow of lava, 150 feet in thickness near the base, scarcely serving to break the great sandstone series. The Archean, where the section terminates, consists of thin-bedded quartzites broken by intrusive veins of a flesh-colored granite, the layers of quartzite standing nearly vertical.

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* For position of these valleys see Atlas of Dutton's Tertiary History of the Grand Cañon, 1882.

The Pre-Tonto series is a remarkable one considering its geologic age. In the Chuar group limestones and shales succeed each other with lithologic characters similar to the Trenton limestone and Utica shale. The parti-colored shales, in one belt 700 feet thick, recall the friable Permian clays. In fact there is no more evidence of metamorphism throughout the 12,000 feet of conformable beds than there is in the evenly bedded strata of the Trias and Cretaceous groups of Southern Utah. Ripple marks and mud cracks abound in many horizons, but not a trace of a fucoid or a molluscan or annelid trail was observed. But for the discovery of a small Discinoid shell, a couple of specimens of a Pteropod allied to *Hyolithes triangularis* and an obscure Stromatopora-like group of forms, the two and one-half months' search for fossils in these groups would have been without result. They serve, however, to retain the group within the Cambrian and also point to a fauna that must be searched for elsewhere, as they alone could scarcely have been the only representatives of the life in the sea at that time.

As now known, the Grand Cañon and Chuar groups may be referred to the lower Cambrian. Their stratigraphic position is essentially the same as that of the Keweenawan group of Wisconsin. Both series were originally deposited over the underlying Archean unconformably. Both were subsequently elevated, eroded, and buried beneath sediments that in each case uncomformably overlie them and also contain a fauna strikingly similar.

Professor Chamberlain, when speaking of the interval between the elevation of the Keweenawan series and the deposition of the Potsdam, suggests that, in part, at least, the Acadian period of the Atlantic border represents a portion of the work of the interval.*

With relation to the interval between the elevation of the Grand Cañon groups and the deposition of the Tonto there is a section in the Eureka District of Central Nevada which passes from the Tonto or Potsdam horizon down through 3,000 feet of conformably bedded limestones before reaching the *Olenellus* horizon. The latter in turn is underlaid conformably by a great belt of quartzite as far down as the section was exposed. These beds appear to have been those deposited in the Nevada sea during the period of land surface over the Grand Cañon area, and if the Keweenawan series correspond to the Grand Cañon group in time, as they certainly appear to do in geologic position, we would consider the interval of erosion before the

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* Geology of Wisconsin, vol. i, p. 94, 1883.

Potsdam as the time of the deposition of the great belt of strata represented by the Pre-Potsdam *Olenellus* horizon of Nevada and Lake Champlain. The older St. Johns and Braintree horizons being correlated, in part at least, with the Grand Cañon and Keweenawan series.

In the Grand Cañon the series is unmetamorphosed, fossiliferous and but slightly disturbed. In Wisconsin the 1,000 feet of lava of the Grand Cañon group is represented by many thousand feet of volcanic rocks, and the coarse sediments were also inimical to the presence of animal life; there were, however, at times, beds of finer material accumulated in a less disturbed sea, and from these we may hope to hear of fossils being obtained.

From the facts presented, both the Grand Cañon and Keweenawan series are considered as belonging to the Cambrian, and united with the Paradoxides horizon of Braintree, Mass., and St. Johns, N. B.; the *Olenellus* horizon of Nevada, Vermont, New York, and Newfoundland, and the Potsdam series of Wisconsin, New York, Canada, etc., as forming the Cambrian age in America, as now known, a subject that will be treated more in detail at some future time. In this arrangement the Lower Magnesian limestone of Wisconsin and the Lower Calciferous of New York and Canada are viewed as passage beds between the Cambrian and Silurian or the first and second faunas of Barrande. In some instances the stratigraphy will necessitate the putting of these beds with the Cambrian and in others with the Silurian. As a whole they are referred to the Silurian as the types of the second fauna predominate very quickly after their first appearance.

Art. L. — *Contributions to Meteorology*; by Elias Loomis, Professor of Natural Philosophy in Yale College. Nineteenth paper, with three plates.

[Read before the National Academy of Sciences, Washington, April 17, 1883.]

The Barometric Gradient in great storms.

In his Meteorological Researches for the use of the Coast Pilot, Part II. Mr. Ferrel gives the following formula for the barometric gradient,

$$G = \frac{1076{\cdot}4\,(2n\cos\psi + v)sP}{\cos i\,(1 + {\cdot}004\,t)\;P'}. \qquad (1)$$

where G denotes the barometric gradient in millimeters per degree of a great circle, or 60 geographic miles,

$2n = \cdot 00014585.$

$\nu = \frac{s \cos i}{r}.$

ψ = the polar distance of the station, or the complement of the latitude.

i = the inclination of the wind's direction to the isobars.

s = the velocity of the wind in meters per second.

r = the distance from the center of the low area expressed in meters.

t = the temperature of the air in centigrade degrees.

P = the pressure of the atmosphere in millimeters.

P′ = 760 millimeters.

Every storm affords an opportunity for testing the accuracy of this formula; but since every storm presents some peculiarities showing the effect of local disturbance for which the formula makes no provision, we cannot expect that the formula will be exactly verified in the case of any storm. If however we determine the average values of the elements of a large number of violent storms, the formula ought to represent these average values. These average values I have endeavored to obtain for the Atlantic Ocean and also for the United States. For the Atlantic Ocean I employed Hoffmeyer's charts, which embrace a period of three years from December, 1873, to November, 1876; but for the first year the observations for the Atlantic Ocean were few, and the isobars generally incomplete. From this series of charts I selected those cases in which the barometric depression was greatest, and in which the isobars were most regular. For these cases I measured with a scale and dividers the distance between the successive isobars (drawn at intervals of five millimeters) and by computation converted these measures into degrees of the meridian. Since the isobars are never entirely symmetrical on all sides, I made my measurements on that side upon which the winds were strongest and the gradients were the steepest. I recorded also the wind's velocity between the various isobars, the inclination of the wind's direction to the nearest isobar, and the state of the thermometer. I recorded the latitude of the low center, and the diameter of the first isobar, except in those cases in which its magnitude was apparently due to the lack of observations from that vicinity. These measurements extended from the lowest isobar up to the highest adjacent isobar, until the winds became feeble and the isobars irregular. The latitude of the center of high pressure was recorded; and if the isobars were tolerably regular, the diameter of the highest isobar was measured. Several of the charts show the isobar 715^{mm}; there are more which have the isobar 720^{mm}; and a large number show the isobar 725^{mm}.

The gradients can generally be measured satisfactorily from the lowest isobar up to the isobar 775 or 780. In order to extend the comparison still further, I selected the cases in which the barometer was highest and the isobars were tolerably symmetrical, and measured the distance between the isobars, continuing the measurements downward until the winds became feeble and the isobars irregular. For the convenience of those who may wish to examine a few of the most remarkable cases, without being compelled to search through the entire series of charts I will give the following examples:

For Low Barometer.	*For High Barometer.*
1875, Jan. 12.	1875, Dec. 30.
Dec. 21.	" 31.
" 22.	1876, Jan. 1.
1876, Jan. 22.	" 2.
March 9.	" 13.
" 10.	

The following table exhibits the average results for the Atlantic Ocean deduced from an examination of 81 cases treated in the manner already described.

TABLE I.—ATLANTIC OCEAN.

Isobars.	Dist. Deg.	G. mm.	Velocity. Sc'le 1-6.	Velocity. Met.	Incli-na'n. Deg.	Therm. centes. Deg.	Radius. Deg.	Radius. Myr'm.	Lat-it'e. Deg.	G′	G″	G‴ co. = 1·3	G^iv	G^v co. = 1·547
715 to 720	1·199	4·17	3·35	15·90	25·4	+ 2·07	2·489	276·6	58·0	3·12		4·06	2·75	4·25
720 " 725	1·292	3·87	3·40	16·21	26·5	2·17	3·735	415·0	57·6	2·91		3·78	2·53	3·91
725 " 730	1·348	3·71	3·39	16·14	27·9	2·17	5·055	561·7	57·2	2·76		3·59	2·39	3·70
730 " 735	1·389	3·60	3·38	16·08	29·9	2·19	6·423	713·7	56·8	2·70		3·51	2·30	3·56
735 " 740	1·420	3·52	3·39	16·14	31·9	2·34	7·828	869·8	56·4	2·70		3·51	2·25	3·48
740 " 745	1·449	3·45	3·41	16·27	33·3	2·69	9·262	1029·1	55·9	2·71		3·52	2·23	3·45
745 " 750	1·484	3·37	3·41	16·27	33·7	3·16	10·729	1192·1	55·5	2·69		3·50	2·20	3·40
750 " 755	1·543	3·24	3·34	15·84	34·1	3·52	12·242	1360·2	55·0	2·60		3·38	2·12	3·28
755 " 760	1·613	3·10	3·16	14·73	34·8	3·84	13·820	1535·5	54·5	2·40		3·12	1·95	3·02
												co. = 1·36		co. = 1·92
760 " 765	1·695	2·95	2·77	12·69	36·9	+ 1·53	13·835	1537·2	54·0	1·90	2·01	2·73	1·61	3·09
765 " 770	1·818	2·75	2·39	10·95	40·7	− 3·99	12·079	1342·1	53·4	1·77	1 87	2·54	1·42	2·73
770 " 775	1·976	2·53	2·14	9·80	44·4	− 9·01	10·182	1131·3	52·9	1·71	1·81	2·46	1·29	2·48
775 " 780	2·165	2·31	1·89	8·67	47·9	−13·65	8·111	901·2	52·2	1·64	1·74	2·37	1·16	2·23
780 " 785	2·370	2·11	1·70	7·82	50·6	−18·09	5·844	648·7	51·5	1·57	1·68	2·28	1·07	2·06
785 " 790	2·604	1·92	1·52	7·02	52·1	−22·40	3·357	373·0	50·7	1·42	1·58	2·15	0·97	1·86

Column 1st shows the isobars between which the distances were measured; column 2d shows the distance between the isobars expressed in degrees of a meridian; column 3d shows the gradient in millimeters per degree deduced from the preceding values; column 4th shows the average velocity of the winds denoted by a scale of 1 to 6. These velocities were reduced to their equivalent values in miles per hour by means of the table prepared under the direction of the Council of the London Meteorological Society and published in 1881. According to this table the values of the numbers employed in the English scale (0–12) are as follows:

Force.	Miles per hour.	Mean.	Force.	Miles per hour.	Mean.	Force.	Miles per hour.	Mean.
0	0 to 5	2·5	5	26 to 30	28·0	10	61 to 69	65·0
1	6 " 10	8·0	6	31 " 36	33·5	11	70 " 80	75·0
2	11 " 15	13·0	7	37 " 44	40·5	12	Above 80	90·0
3	16 " 20	18·0	8	45 " 52	48·5			
4	21 " 25	23·0	9	53 " 60	56·5			

The average values of the numbers upon the scale 0–6 are hence determined to be as follows:

Force.	Miles per hour.	Metres per second.	Force.	Miles per hour.	Metres per second.
1	10·5	4·69	4	44·5	19·89
2	20·5	9·16	5	60·75	27·16
3	30·75	13·75	6	82·5	36·88

By means of this table the numbers in column 5th were deduced from those in column 4th. Column 6th shows the inclination of the winds to the nearest isobar; column 7th shows the temperature of the air in centesimal degrees; column 8th shows, for isobars less than 760^{mm}, the distance from the center of the low area to a point midway between the isobars mentioned in column 1st; and, for isobars above 760^{mm}, it shows the distance from the center of the high area. The average diameter of the first isobar about the low center was 3·78 degrees; and the average diameter of the first isobar about the high center was 4·11 degrees. The distances in column 9th are expressed in myriameters. The average latitude of the centers of low barometer employed in constructing this table is 58°·8; and the average latitude of the centers of high barometer is 49°·7. In computing the gradient for the different heights of the barometer, it was assumed that in going from the low center to the high center the change of latitude was proportional to the distance passed over. Upon this principle were computed the numbers in column 10th which represent the latitudes corresponding to the points midway between the isobars named in column 1st.

Column 11th shows the gradients computed by Ferrel's formula from the data in columns 1–10. For pressures above 760^{mm}, the second term of the formula was assumed to be negative. The numbers in column 12th denote the gradients computed by using only the first term of the formula. The mode of obtaining the other columns will be explained hereafter. This table readily suggests several important conclusions, but I will defer considering them until I have obtained the corresponding results for the United States.

The lower part of Plate I gives a graphic representation of the numbers in the preceding table. At the center of low pressure the barometer stands at 712^{mm}, and about this center the isobars are drawn at intervals corresponding to the numbers in column 2d. The isobars are continued up to the center of high pressure where the barometer stands at 792^{mm}. The entire range of pressure shown on the chart is 80^{mm} or 3·15 English inches. The arrows represent the directions of the wind shown in column 6th. The figure next above the preceding shows the corresponding gradients from the low center to the high center. The distance from the low center to the high center is 29·31 degrees. The entire figure represents a length of 58·62 degrees, or 4,045 English miles.

In order to obtain the average elements of the violent storms of the United States, I employed the Weather Maps of the Signal Service. I have a complete set of the tri-daily weather maps from Nov., 1871, to the close of 1880; and I have also one daily map from the beginning of 1881 to the present time, making in the aggregate about 11,000 maps. The isobars on these maps show much greater irregularities than those upon Hoffmeyer's charts for the Atlantic Ocean. This difference may be ascribed in part to the greater number of observations, and the attempt to draw the isobars so as to be consistent with all the observations; but a large part of the difference appears to be due to local disturbing influences which are more numerous over a continent than over the ocean. In order to obtain suitable data for testing Ferrel's formula, we must in some way eliminate the influence of these local disturbing causes. If we should take an indiscriminate average of the gradients, wind velocities, etc., for all the storms, it would require an immense number of cases to furnish consistent results; and it is doubtful whether even these results would be as satisfactory as results derived from a small number of cases of violent storms which were less influenced by local causes. I have accordingly examined with great care my entire series of weather maps and have selected those cases in which the barometric depression was greatest and the isobars were most regular, and I have restricted my comparisons to that side of each storm upon which the winds were strongest, and the gradients were steepest. The comparisons were conducted in the manner already described for the Atlantic Ocean. For the convenience of those who may wish to examine some of the most remarkable cases I give the following list.

For Low Barometer.						*For High Barometer.*					
1874,	Nov.	22.3	1876,	Dec.	29.3	1872,	Dec.	26.2	1879,	Feb.	26.1
	"	23.1	1878,	"	10.2	1873,	Jan.	15.1		March	13.3
1875,	Jan.	2.3		"	22.1		"	15.2		Dec.	21.3
	March	15.2	1879,	Jan.	2.3		"	27.3		"	22.1
	Nov.	11.1		"	3.1		"	28.1		"	23.3
	"	17.3		"	3.2	1875,	Nov.	28.2	1880,	Jan.	30.1
	"	29.2		"	3.3	1876,	Nov.	30.3		Feb.	19.2
	"	29.3		"	4.1		Dec.	8.2		"	19.3
1876,	Jan.	9.2		"	10.1		"	14.1		"	20.1
	Feb.	2.1		"	26.2	1877,	Feb.	13.1		"	27.2
	"	2.2		Feb.	21.1		"	13.2		March	11.2
	"	15.2		"	21.2	1878,	Dec.	24.1		"	11.3
	"	25.2		"	21.3		"	24.2		Nov.	23.3
	March	21.1	1880,	Feb.	11.3	1879,	Jan.	2.2		Dec.	26.3
	Dec.	8.3		Oct.	16.1		"	3.1		"	29.2
	"	9.2		"	16.2		"	4.1	1881,	Feb.	6.1
	"	9.3	1882,	Jan.	2.1		"	26.1	1882,	Feb.	18.1
							Feb.	25.3		Dec.	7.1

The following table exhibits the average results for the United States deduced from an examination of 123 cases treated in the manner previously described. For convenience of comparison with the data for the Atlantic Ocean, the results have all been converted into French units, with the exception of the numbers in column 1st.

TABLE II.—UNITED STATES.

Isobars.	Dist. Deg.	G. mm.	Veloc Met.	I'cli-na'n Deg.	Therm. centes. Deg.	Radius.		G^{i}	G^{ii}	G^{iii} co.= 1·645	G^{iv}	G^{v} co.= 2·218
						Deg.	Myr'm.					
28·8 to 29·0	1·362	3·73	12·20	36·8	−2·22	2·241	249·0	2·27		3·73	1·72	3·81
29·0 " 29·2	1·511	3·36	11·80	38·3	−2·06	3·677	408·5	1·99		3·27	1·50	3·33
29·2 " 29·4	1·651	3·08	11·44	39·8	−1·89	5·258	584·2	1·86		3·06	1·39	3·08
29·4 " 29·6	1·749	2·90	11·09	41·0	−1·72	6·958	773·1	1·78		2·93	1·32	2·93
29·6 " 29·8	1·816	2·80	10·64	42·4	−1·94	8·741	971·2	1·72		2·83	1·25	2·77
29·8 " 30·0	1·867	2·72	10·28	43·3	−3·22	10·582	1175·8	1·68		2·76	1·20	2·66
										co.= 1·63		co.= 2·362
30·0 to 30·2	1·945	2·61	9·66	44·4	− 6·17	9·586	1065·1	1·45	1·54	2·51	1·10	2·60
30·2 " 30·4	2·049	2·48	9·03	45·2	−10·11	7·589	848·2	1·39	1·50	2·45	1·06	2·50
30·4 " 30·6	2·203	2·31	8·18	47·0	−14·11	5·463	607·0	1·31	1·43	2·33	·98	2·31
30·6 " 30·8	2·427	2·09	7·20	49·2	−17·39	3·148	349·8	1·17	1·35	2·20	·88	2·08

The average latitude of the centers of low barometer was 44°·7, and the average diameter of the 1st isobar about the low center was 3·12 degrees. The average latitude of the centers of high barometer was 45°·1, and the average diameter of the 1st isobar about the high center was 3·87 degrees. Since the average latitude of the centers of low and high pressure is so nearly the same, in computing the gradients by the formula, the latitude of each center was taken to be 45°.

The upper part of Plate I gives a graphic representation of the numbers in the preceding table, and is constructed in the same manner as the lower figure on the same plate. At the center of low pressure the barometer stands at 28·7 inches; and at the center of high pressure the barometer stands at 30·9 inches, showing a range of pressure amounting to 2·2 inches. The distance from the low center to the high center is 22·075 degrees, and the entire figure represents a length of 44·15 degrees, or 3046 English miles.

Upon comparing table I with table II, the first fact which attracts our attention is that the gradients in the two cases do not differ very greatly, although the velocities of the wind are very unequal. For low barometer, the average gradient over the Atlantic Ocean exceeds the gradient over the United States only fifteen per cent, while the average velocity of the wind in the former case exceeds the velocity in the latter case by forty per cent. This fact indicates that the barometric gradient is determined not mainly by the wind which prevails at the earth's surface, but by that which prevails at an elevation such that its velocity is not sensibly affected by the resistance arising from the inequalities of the earth's surface.

The second fact which attracts our attention is that over the Atlantic Ocean, violent storms have a much greater geographical extent than they have over the United States. For the United States the average diameter of the area of low pressure (below 30 inches) in the case of violent storms is 23 degrees, or 1587 English miles. Over the Atlantic Ocean it is 29·3 degrees or 2022 miles, which is 27 per cent greater than for the United States; and since the gradients are fifteen per cent steeper, the depression of the barometer at the center of a violent storm is about 40 per cent greater over the Atlantic Ocean than over the United States. This difference is a permanent one and must be ascribed to a permanent cause. In the case of a great barometric depression, there is a powerful force in operation, near the central portions of this area, which draws in the surrounding air from a great distance. The greater resistance experienced by the air in its motion over the land than over the water, may perhaps partially explain the less extent of the low area in the United States. But we find that in the interior of Europe violent storms have nearly the same geographical extent as over the Atlantic Ocean. This is shown by Hoffmeyer's Weather Charts and by the International Weather Maps. Over the western portion of Europe, near the Atlantic Ocean, the barometer frequently falls as low as 720^{mm}, and sometimes it falls even lower than this at a great distance from the ocean. In 1880, Feb. 29th, in lat. 61¾° N. and long. 25° E. the barometer fell to 719^{mm}, and in 1881, March 19th, in lat. 64° N. and long. 39½° E. it fell to 709^{mm}. The last storm was remarkable for an increase of intensity in its progress eastward over the continent. When it left the Atlantic Ocean the pressure at the center of the storm was 728^{mm}, and the pressure fell to 709^{mm} (27·9 inches) as the storm advanced eastward. Over the United States the barometer never falls so low. In my eighth paper I gave a list of all the cases in which, at any of the Signal Service stations, the barometer fell as low as 29·25 inches during a period of two years. This list shows only twenty-eight cases in which the barometer fell below 29·00 inches; it shows sixteen cases in which the barometer fell below 28·9 inches; it shows five cases in which the barometer fell below 28·8 inches, and only three cases in which it fell below 28·7 inches. These three cases occurred Nov. 18, 1873, at the first, second and third observations for that day, the lowest pressure being 28·47 inches at Eastport, Me. According to the record of Mr. R. T. Paine, the lowest point which the barometer at Boston has reached during the past sixty years, is 28·47 inches (Nov. 25, 1846), and this is probably as low as the barometer has fallen in that part of New England at any time since reliable barometric observations have been made. There is no doubt that the geographical extent of areas of low pressure is considerably less in the United States than it is in Northern Europe, and I think the cause is to be found in the permanent ridge of high pressure prevailing in the southern portion of the United States. My twelfth paper was accompanied by a chart showing that during the colder months of the year there is a ridge of high pressure which is extremely persistent between the parallels of 30° and 35°. When the center of a great storm is near lat. 45°, the area of low pressure seldom extends much below the parallel of 35°. It may be inferred that if a storm center should pass farther north, the storm would attain greater dimensions and there would be a greater depression of the barometer at the center. This conclusion is certainly correct for the eastern border of the American continent, particularly for the neighborhood of Greenland; and it is probably true (though in a less degree) for the interior of the continent; but on account of the lack of observations we cannot make a very satisfactory comparison.

The column marked G′ in table I was computed from Ferrel's formula, assuming that for pressures above 760^{mm}, the second term of the formula should be taken with a negative sign. A careful examination of this table will show that for pressures above 760^{mm} the second term should be omitted entirely. It is evident that if the circulation of the air about a center of high pressure gives rise to a centrifugal force, its effect must be to diminish the gradient. When we go from a center of low pressure to a center of high pressure, the second term of the formula must therefore change its sign on passing the line of mean pressure. This must cause an abrupt change in the computed values of the gradient, at a point where we pass from the low area to the high area. Such a change will be noticed in the values of G′ in table I. But the observed values of the gradient show no such change. The differences between the successive values of the gradient given in column third of tables I and II, show no abrupt change marking the passage over the line of mean-pressure. We therefore conclude that by the circulation of the air around a center of high pressure no appreciable centrifugal force is developed. We also conclude that when there is a decided centrifugal force in the central portion of an area of low pressure, this centrifugal force does not extend in an appreciable degree as far as the line of mean pressure. This conclusion is indicated not merely by the numbers in column third of the tables, but by the form of the isobar of 760^{mm} or 30 inches, which separates a low from a high area. This isobar, instead of forming an arc of a circle drawn about a center of low or high pressure, always inclines towards the form of a straight line. This fact may be verified by the examination of any weather map showing the progress of a violent storm; and it is seen on various charts accompanying my preceding papers, e. g., paper ninth, storm of April 12, 1874; paper twelth, storm of Jan. 15, 1877, and paper thirteenth, storm of April 11, 1874. The best results to be derived from Ferrel's formula are obtained by combining the numbers in the column G′ for pressures below the mean, with the numbers in the column G″ for pressures above the mean.

Over the Atlantic Ocean for pressures less than 760^{mm}, the average gradient computed by the formula is only 77 per cent of the observed gradient; and for pressures greater than 760^{mm} the average gradient computed by the formula (omitting the last term) is only 73 per cent of the observed gradient. If we multiply the gradients computed for an area of low pressure by 1·3, we obtain the first nine numbers in column thirteenth of table I, and if we multiply the gradients computed for a high area by 1·36 we obtain the last six numbers in column thirteenth. We perceive that the differences between the numbers in columns G and G‴ are not very great.

Over the United States, for pressures less than 30 inches, the average gradient computed by the formula is only 61 per cent of the observed gradient, and for pressures greater than thirty inches, the average gradient computed by the formula (omitting the last term), is also about 61 per cent of the observed gradient. If we multiply the gradients computed for an area of low pressure by 1·645, we obtain the first six numbers in column eleventh of table II; and if we multiply the gradients computed for a high area by 1·63 we obtain the last four numbers in column eleventh. We perceive that the differences between the numbers in columns G and G''' are less than they were in table I.

This comparison indicates that the resistance encountered by the air, even over the ocean, is considerably greater than that assumed in the formula. Having discovered that the coëfficient sec. *i*, employed in Ferrel's formula, does not adequately represent the resistance which the air near the earth's surface experiences in violent storms, I endeavored to modify the formula so as to represent the observations more accurately, and found that the following formula

$$G = \frac{\cdot 157\, v \sin.\, \varphi + 1076 \cdot 4 \dfrac{(v \cos.\, i)^2}{r}}{(1 + \cdot 004\, t)} \cdot \frac{P}{P'} \qquad (2)$$

gave very good results when combined with a suitable coëfficient. Column fourteenth in table I, shows the numbers computed from this formula, the last six numbers being obtained by employing only the first term of the formula. By multiplying the first nine numbers by 1·547 and the last six numbers by 1·92, we obtain the numbers in column fifteenth, and we perceive that these numbers agree with the observed gradients given in column third better than the numbers computed by Ferrel's formula. In the latter case the average error of the computed gradients (disregarding their algebraic signs), is 0·12, while in the former case it is only 0·05.

Applying the same formula to table II, we obtain the numbers in column twelfth; and if we multiply the first six numbers by 2·218 and the last four numbers by 2·362, we obtain the numbers in column thirteenth, and we perceive that these numbers accord very well with the observed gradients given in column third. For both table I and table II, the average error of formula (1) (disregarding the algebraic signs) is 0·088^{mm}, while the average error of formula (2), is only 0·041^{mm}.

It may be thought desirable to ascertain how far the preceding formulas will represent the phenomena of a particular storm. For this purpose I have selected the remarkable storm of Feb. 5, 1870, of which an account has been given by Capt. Toynbee in No. 13 of the Publications of the English Meteorological Council. Besides the observations given in Captain Toynbee's Memoir, I have endeavored to obtain additional observations both for the Atlantic Ocean and the adjacent continents. For the Atlantic Ocean I have met with small success, but for the continent of Europe there are published observations which give very complete information respecting the condition of the atmosphere on the eastern side of the area of low pressure. Nos. 1–18 of table III show (with one exception) all the observations contained in Captain Toynbee's Memoir derived from vessels navigating the Atlantic Ocean and reporting a pressure less than 30 inches on Feb. 5th at 7^h 35^m P. M., Greenwich time, this being the instant of lowest observed pressure. The observations of the vessel Carl Georg are rejected as being irreconcilable with the other observations. The observations for each of these vessels are shown in table III, arranged in the same manner as in tables I and II. Instead of applying the formulas to each of these observations separately, I have divided the observations into four groups, and taken the average value of the corresponding data for all the vessels included in a group, and the results are given in table V. No. 1 indicating a pressure much less than any other vessel is allowed to stand by itself and forms the first line of table V. Nos. 2–6 showing pressures between 28·0 and 28·5 inches form the second group, and the average of the observations for these five vessels is given in the second line of table V. Nos. 7–11 showing pressures between 28·5 and 29·0 inches form the third group and the averages are given in the third line of Table V. Nos. 12–18 showing pressures between 29·0 and 30·0 inches form the fourth group and the averages are

TABLE III. ATLANTIC OCEAN, 1870, FEB. 5, 7^h 35^m P. M., GREENWICH TIME.

No.	Vessel.	Lat.		Long.		Barom m. m.	Therm cent.	Wind.			Gradient. mm. 1°.	Inclination.	Radius Myriam.
								Direct'n	Force.	Velocity ml. p. s.			
1	Tarifa	51°	3′	23°	59′	694·2	11°·7	S 11° W	10	29·1	18·0	12°	76·4
2	Weser	49	25	25	8	715·0	8·9	S 86 W	12	40·2	8·4	6	281·1
3	Marathon	50	45	21	34	718·3	11·7	S 11 W	10	29·1	9·1	8	275·6
4	City of Cork	50	16	30	15	720·6	3·3	N 34 W	9	25·3	5·6	—10	482·2
5	Orontes	50	47	20	57	721·3	11·1	SW	10	29·1	8·1	—25	318·9
6	Minnesota	51	12	20	40	723·1	....	S 11 W	9	25·3	7·9	5	318·9
7	C. Antwerp	49	45	28	54	726·9	10·0	N 56 W	10	29·1	6·1	10	415·5
8	India	51	53	33	33	728·7	....	NW	6	15·0	3·0	11	692·2
9	England	51	5	19	0	729·0	8·9	S 17 W	10	29·1	6·9	10	454·4
10	Queen	51	4	17	54	734·0	11·1	S 34 W	9	25·3	5·3	—10	541·1
11	Nestorian	55	13	14	7	734·8	9·4	S 28 E	9	25·3	4·1	48	875·5
12	Venezuelan	46	3	29	42	739·1	7·8	N 79 W	9	25·3	4·1	7	794·4
13	Helvetia	47	12	38	53	740·1	2·2	N 79 W	9	25·3	2·5	25	1283·2
14	Cuban	45	58	22	23	740·6	7·2	S 79 W	9	25·3	4·8	0	709·9
15	Westphalia	47	5	39	30	740·6	2·2	S 74 W	9	25·3	2·5	48	1339·9
16	Prussian	48	11	41	31	743·2	0·6	West	7	18·1	2·5	48	1433·2
17	Denmark	48	37	36	11	747·0	0·6	N 79 W	7	18·1	2·8	26	1004·3
18	Nile	44	44	13	11	760·5	....	SW	7	18·1	2·8	3	1281·0

given in the fourth line of table V. Table IV, Nos. 19–24 hows observations for the same instant from six stations in reland, Scotland and neighboring islands, and the averages for hese stations are given in the fifth line of table V.

TABLE IV. LAND OBSERVATIONS, 1870, FEB. 5, 7ʰ 35ᵐ P. M.

No.	Station.	Lat.	Long.	Barom m. m.	Th'm cent.	Wind. Direct'n	Velocity mi. p. h.	Velocity mi. p. s.	Gradient. mm. 1°.	Inclination.	Radius Myriam.
19	Valencia	51° 55′	10° 18′	749·8	7°·8	S 22° E	37	16·5	3·8	55°	1063·2
20	Armagh ...	54 21	6 39	755·1	3 ·9	SE	12	5·4	3·8	72	1317·6
21	Glasgow	55 53	4 18	756·9	5 ·0	S 34 E	8	3·6	3·56	63	1498·7
22	Aberdeen ...	57 10	2 6	756·9	5 ·0	S 22 E	13	5·8	3·30	49	1657·6
23	Skye	57 29	6 34	752·3	4 ·4	SE	28	12·5	3·56	58	1384·3
24	Thorshaven..	62 2	6 44	750·6	5 ·0	South	33½	15·0	2·79	0	1601·0

TABLE V. AVERAGE RESULTS.

	Lat.	Barom. mm.	Therm. cent.	W. Velocity. m. p. s.	Gradient. mm. 1°.	Inclination.	Radius Myriam.	G′	G″ c=1·32	G‴	G'''' c=1·32
Tarifa.	51° 3′	694·2	11° 7	29·1	18·0	0°	76·4	13·68	18·06	13·68	18·06
28·0–28·5	50 29	719·7	8 ·7	29·8	7·8	0	335·3	5·91	7·80	5·91	7·80
28·5–29·0	51 48	730·7	9 ·8	24·8	5·1	6	595·8	3·87	5·11	3·85	5·08
29·0–30·0	46 50	744·4	3 ·4	22·2	3·1	22	1121·0	3·12	4·10	2·85	3·76
Land obs											
29·5–30·0	56 28	753·6	5 ·2	9·8	3·5	50	1420·5	2·01	2·65	1·28	1·69

In three of these cases the direction of the wind as reported was *outward* from the center of the low area, and for the first six cases the average inclination of the wind to the isobars was zero. I have therefore inserted in table V the inclination for the first two lines as zero, although the wind reported by the Tarifa was inclined 12° inward. Column 9th in table V shows the gradients computed by formula (1) and column 10th shows the values in column 9th multiplied by 1·32. Column 11th shows the gradient computed by formula (2), and column 12th shows the preceding values multiplied by 1·32. It will be observed that both formulas give the same result in the first two lines, as should be the case since the formulas become identical when $i=0$. For the first three lines the computed gradients (multiplied by 1·32) agree well with the observed gradients; but for the fourth line the computed gradients are too great, and for the fifth line they are too small. The wind velocities reported by the vessels 12–18 are greater than are usually found associated with the given gradients; and the velocities at the stations 20–22 were less than are usually associated with the gradients existing at that time. This example shows how a general formula (although founded on correct principles) may fail to represent accurately the gradients for the separate observations of any particular storm. The close agreement of the computed gradients with the observed gradients in the first three lines of the table must be regarded as highly satisfactory; and the discrepancies found in lines 4 and 5 show that a close agreement between the observed and computed gradients is to be expected only when the data compared are deduced from the average of a large number of observations.

The accompanying map shows the isobars for the storm here investigated. The curves were first drawn with great care upon a map whose scale was more than three times as large as that of the map here given. For the southern half of the low area the position of the isobars is well determined. For the northern half of the low area the number of observations is small, and the exact position of the isobars cannot be certainly assigned; but this uncertainty does not materially affect the conclusions derived from the observations in table III. During the progress of this storm, there was an area of high pressure prevailing over the northeast part of the United States, a second area of high pressure prevailed along the northwest coast of Africa; and a third area in the northeast part of Europe. As the pressure in the latter case was unusually high, I have collected observations sufficient to determine the position of the isobars. Table VI shows the observations from 98 stations derived from the official publications of Russia, Austria, Sweden, Norway, etc. Some of the observations were derived from the Bulletin de l'Observatoire de Paris. As these observations are only given for 8 A. M., I have taken the mean of the barometric observations for Feb. 5 and 6. Since the change of pressure during these 24 hours was generally less than 6^{mm} the error of the result thus obtained cannot be very great. The isobars to which the greatest importance is attached are however derived solely from observations made near 8 P. M. The barometric observations have all been reduced to sea-level. In the Russian observations the force of the wind is expressed by the scale 1–10; in the Norwegian observations the scale is 1–6; and in the Swedish observations it is 1–4.

TABLE VI. EUROPE, 1870, Feb. 5, 8 P. M., Greenwich Time.

tation.	Lat.	Long.	Barom. sea level.	Therm. Fahr.	Direct. wind.	Force.
	70° 22′	31° 7′	29·88	26	W.	4
	69 39	18 58	30·00	39	S.W.	1
	67 17	14 24	30·12	33	E.	1
da	65 51	24 11	30·40	14	S.	2
	65 28	12 14	30·17	35		0
	64 57	34 39	30·60	— 3	S.W.	4
elsk	64 33	40 32	30·94	—10	S.	6
sund	63 7	7 45	30·03	43	S.S.E.	2
nd	62 38	17 57	30·50	21	S.S.W.	2
d	62 29	6 9	30·03	41	E.S.E.	2
	61 36	5 2	30·05	39	E.S.E.	2
	61 6	7 27	30·31	36	E.	3
	60 53	11 33	30·59	14		0
	60 36	15 37	30·51	15		0
	60 24	5 20	30·15	38	S.S.E.	2
ors	60 10	24 57	30·89			
dt	59 59	29 47	31·00	—16	S.E.	4
rsburg	59 56	30 16	31·09	—16		0
ia	59 55	10 45	30·58	22		0
	59 26	24 45	30·98	—12	S.	4
	59 23	13 30	30·67	21	S.	1
Port	59 21	24 3	31·00	—11	S.	4
lm	59 20	18 5	30·71	21	S.	1
es	59 9	5 16	30·24	33	E.S.E.	4
nd	59 5	10 27	30·57	23	E.	2
	58 23	26 43	31·07	—22	S.E.	2
	58 2	7 27	30·47	27	E.S.E.	4
ik	57 46	16 38	30·61	19		0
g	57 42	11 58	30·60	18	S.E.	1
	57 39	18 19	30·71	14	S.S.E.	1
	56 58	24 6	30·92	—15	S.E.	4
	56 40	16 21	30·74	16	S.E.	1½
d	56 40	12 51	30·51	19	S.E.	3
	56 39	23 44	30·93	—15		0
	55 56	13 8	30·50	15	E.S.E.	2
	55 46	37 40	30·98	—15	N.E.	2
	54 41	25 18	30·77	—17	E.	2
den	53 12	5 50	30·10	27	E.S.E.	2
ue	53 12	6 34	30·11	26	S.E.	2
	52 58	4 45	30·02	33	E.S.E.	2
u	52 13	21 1	30·71	—13	E.	4
	52 5	5 7	30·10	34	S.E.	1
	51 45	1 16w.	29·94	42		
ch	51 29	0 0	29·85	43	S.	3
en	51 25	3 35	30·02	42	S.	3
	51 20	12 24	30·32		E.	1
ue	51 2	2 22	30·06	41	S.S.E.	1
ht	50 51	5 41	29·85	41	N.E.	2
	50 51	4 22	30·01	45	S.S.W.	1
e	50 44	1 30	29·90	39	S.S.W.	1
	50 26	31 31	30·65	—18	N.	2
	50 5	14 25	30·56	13		
	50 4	19 57	30·66	—15		
g	49 50	24 2	30·66	—19		
rs	49 46	4 44	30·06	34	E.	1
rg	49 38	1 36w.	29·97	39	S.	2

TABLE VI—*Continued.*

Station.	Lat.	Long.	Barom. sea level.	Therm. Fahr.	Direct. wind.	Force.
Havre	49° 31′	0° 6′	29·98	41	S.E.	1
Paris	48 50	2 20	30·02	40		0
Versailles	48 48	2 7	29·94	46	S.W.	1
Beauficel	48 45	0 57w.	29·97	43	S.W.	1
Lugan	48 35	39 20	30·67	— 3	E.	6
Strasbourg	48 35	7 45	30·20	22		0
Wien	48 12	16 22	30·38	10	E.	1
Lorient	47 44	3 21w.	29·93	40	W.N.W.	1
Debreczin	47 32	21 41	30·36	— 3		
Besançon	47 14	6 1	30·12	32	N.E.	1
Kischinef	47 1	28 50	30·46	—11	N.	2
Nikolajef	46 58	31 58	30·41	— 9	N.	2
Berne	46 57	7 26	30·11	26		
Napoleon Vendee	46 40	1 38w.	80·09	41	N.E.	1
Odessa	46 29	30 44	30·58	—11	N.E.	4
Scegedin	46 15	20 8	30·42	11		
Dniestrowski	46 5	30 29	30·66	— 5	N.E.	8
Rochefort	45 56	0 58w.	29·94	44		
Limoges	45 51	1 14	29·99	41	S.W.	1
Agram	45 49	15 55	30·20	22		
Lyon	45 45	4 49	30·18	41	S.	1
Triest	45 39	13 46	30·14	38		
Turin	45 11	7 41	30·18	34		1
Pola	44 52	13 50	30·12	44		
Bordeaux	44 50	0 32w.	30·00	43		
Sewastopol	44 37	33 33	30·26	14	N.E.	4
Montauban	44 1	1 20	30·10	41	S.W.	1
Florence	43 46	11 14	30·16			
Ancona	43 36	13 33	30·17	39		
Bayonne	43 30	1 30w.	29·98	43		
Cette	43 23	3 40	30·07	43	N.E.	1
Marseille	43 18	5 23	30·04	43	S.E.	1
Bilbao	43 15	2 59w.	30·07			
Lesina	43 11	16 27	30·09	44		
Civita Vecchia	42 4	11 45	30·02	50	S.E.	2
Rome	41 54	12 29	30·03	47		0
Barcelona	41 23	2 11	30·03			
Durazzo	41 19	19 28	30·15	36		
Oporto	41 9	8 37w.	30·33			
Palma	39 40	2 40	29·96			
Palermo	38 7	13 22	29·99			
Tarifa	36 3	5 35w.	30·28			

The following table shows the state of the barometer, etc., at several additional stations, most of them being situated in the northern half of the low area.

On the accompanying map, the isobars are drawn at intervals of two-tenths of an inch from 31 inches to 27·4 inches. The direction of the winds is also shown for a considerable number of stations, and its force is indicated by the number of feathers on the arrows (scale 1–6).

We cannot well avoid the conclusion that an area of high essure so remarkable as 31 inches situated so near to an area

Station.	Latitude.	Longitude.	Barometer.	Thermometer. Fahr.	Wind.	
					Direction.	Force.
ine Island	74° 32′	18° 49′	29·61	−2		
obshavn	69 13	50 55	29·54	3	NE	
kkisholm	65 5	22 46	28·92	34	East.	7
rison Pt.	64 55	67 15	29·98	−30	WNW	2
kiavig	64 20	22 0	28·62	39	NE	
lthaab	64 11	51 46	29·29	16	NE	4
ctut	61 12	48 11	29·19	25	West.	2
th Unst	60 45	0 55	29·74			
John's	47 30	53 15	30·10	39	SW	1
ceira	38 39	27 15	30·01		SW	

low pressure still more remarkable is not the result of acci- nt. From Jan. 29th to Feb. 8th a low area of unusual mag- ude prevailed over the Atlantic Ocean, and during all this ie an area of high pressure prevailed over Europe with un- ıal persistence and at length attained an extraordinary mag- ude. At St. Petersburgh the barometer rose steadily from 4^{mm} on the 29th of January to 790^{mm} on the 6th of February. e know that within a low area the air ascends, and that thin a high area it descends. It seems natural to conclude it the air which ascended from the low area prevailing over Atlantic Ocean descended in the high area which prevailed er Northeastern Europe. According to this view, the high a would be (at least in part) the *effect* of the low area. The tward flow of air from the high area must supply the inward w of some low area. This high area in Northeastern Europe, b. 5th seems to have supplied in some degree the air for the v area over the Atlantic, but it could not have been the prin- ıal supply, for the movement of the air from the high area s sluggish. But on the 6th of February the outward move- nt from the west side of the high area became stronger, and s movement became still more decided on the 7th and 8th, which time the low center had reached the coast of Ireland. ıus high areas appear to be built up by low areas, and high as become *feeders* to low areas.

Table I, page 444 will enable us to construct the curve which resents the path of a particle of air in its progress towards center of an area of low pressure. Column 6th shows the lination of this curve to the isobars at a series of points dway between the isobars shown in column 1st. Plate III hibits these curves for an area of low pressure and also for an a of high pressure. There are certain important particulars in which this figure does not correspond to nature. The isobars about a low center in a great storm are never exact circles; and when an area of high pressure meets an area of low pressure the isobar of 760^{mm} tends to assume the form of a straight line.

We see from this figure that the path of a particle of air in its progress towards a low center is *not* a logarithmic spiral. The angle which this curve forms with the isobars is not constant, but diminishes in approaching the center of the low area. This change in the angle of inclination is not simply an occasional occurrence, but is an *invariable* characteristic of great and violent storms.

In my 2d and 8th meteorological papers I gave a table showing the mean distance between the isobars in the neighborhood of a storm's center (the isobars being drawn at intervals of one-tenth inch) for all velocities of the wind from zero to 50 miles per hour, according to the United States Signal Service maps. If for these velocities we compute the corresponding gradients we shall find that the results do not accord well with the preceding formulas. According to these formulas the gradient should be zero when the velocity of the wind is zero; and for a given locality the gradient should increase nearly in the same ratio as the velocity of the wind. But according to the tables above mentioned, when the velocity of the wind is zero, the gradient amounts to 0·075 inch for a distance of 100 miles, and for a velocity of 50 miles per hour the gradient is only three times as great as for a velocity of zero. These anomalies result partly from the mode in which the tables were constructed. A sheet of paper was ruled with 40 vertical columns and at the head of the columns were placed the numbers 0, 1, 2, 3, etc., to denote the velocity of the wind. For each station within an area of low barometer the distance between the two adjacent isobars was measured, and the distance recorded under the corresponding velocity found at the top of one of the vertical columns. Now it frequently happens that the velocity of the wind at some station is zero; but according to the Signal Service maps the gradient is never zero. Indeed it frequently happens that at some station the velocity of the wind is zero when the gradient is very steep. The following examples taken from the volume of observations for September, 1877, will show the truth of this statement. In each of these cases the velocity of the wind was reported to be zero.

It is evident that in these cases the gradients were not the result of a calm, but they resulted from a strong general movement of the atmosphere, and the calms reported at certain stations were exceptions to the general movement of the atmosphere and resulted from local causes. If we combine these steep gradients with the small gradients which usually accompany feeble winds we obtain an average gradient which is considerably too large.

Date.	Station.	Barometer.	Gradient to 1°.
1877, Sept. 3.3	St. Paul, Minn.	29·80	0·070 inch.
4.1	St. Paul, Minn.	·80	·097
10.1	Knoxville, Tenn.	·89	·065
12.3	Duluth, Minn.	·90	·075
13.3	Duluth, Minn.	·70	·097
21.1	Albany, N. Y.	·90	·062
22.1	Duluth, Minn.	·78	·080
28.3	Ft. Sully, Dak.	·42	·092

On the other hand we sometimes find stations where the wind is very high and the gradient is small. If we examine the 250 cases of winds amounting to 40 miles per hour reported in my 8th paper we find that in several of them the gradient was very small. This will be seen from the following list where column sixth shows the gradient to one degree expressed in decimals of an inch.

Date.	Station.	Barom.	Wind.		Gradient to 1°.
			Direct'n.	Veloc'y.	
1872, Oct. 7.1	Indianola, Tex.	29·99	N	43	0·053 inch.
1873, Jan. 8.2	Key West, Fla.	30·03	N	40	·059
16.2	Punta Rassa, Fla.	29·93	NW	41	·049
Feb. 22.3	Indianola, Tex.	30·17	N	48	·049
Nov. 5.2	Galveston, Tex.	30·05	N	40	·052
12.1	Galveston, Tex.	30·21	N	40	·052
Dec. 9.1	Key West, Fla.	30·18	NE	40	·052
12.3	Indianola, Tex.	30·22	N	40	·052
1874, Jan. 4.1	Indianola, Tex.	30·12	N	48	·063
4.2	Indianola, Tex.	30·25	N	40	·063
Apr. 17.1	Indianola, Tex.	30·05	N	40	·063

It will be noticed that all these stations are on the coast of the Gulf of Mexico; that in each case the wind was northerly, and all (with perhaps one exception) occurred during the prevalence of what is popularly known as a *norther*. Now when we combine these cases of small gradients with the cases of steep gradients which usually attend a wind of forty miles per hour, the average value of the gradient is very much reduced. Thus we find that by combining in a common average the observations from all portions of the United States in the manner adopted in my second paper, we obtain an average gradient which is too great for the small velocities of the wind, and too small for the high velocities. For a velocity of the wind amounting to about twenty-two miles per hour, the gradients found in my second paper, accord very well with those shown in table II on page 447 of the present paper. These results then, when carefully examined, do not appear to be inconsistent with the results of the present article; but in order to test in a satisfactory manner the formulæ here given, the observations should be classified so as to show, not merely the velocity of the wind corresponding to a given gradient, but also the barometric pressure, the temperature of the air, the inclination of the wind to the isobars, and the distance of each station from the center of low pressure. It also seems undesirable to combine in a common average observations from stations which differ greatly in their local peculiarities.

Several European meteorologists have determined the barometric gradient corresponding to different velocities of the wind, by comparing observations at a single station with observations at neighboring stations. Messrs. Whipple and Baker have determined the barometric gradients at Kew by comparing the Kew observations for five years with observations made at other stations distant about 100 miles. The gradients thus determined accord well with the results of the present paper, as will be seen from the following table, in which column first shows the velocity of the wind expressed in meters per second, and column second shows the corresponding gradient expressed in millimeters for a distance of one degree. The inclination of the wind to the isobars was found to be nearly the same for all velocities of the wind and is taken equal to 38°. The distance of Kew from the centers of low pressure is not given, but since 60 per cent of all the observations were employed, it is presumed that in about half of the cases the pressure was above the mean, and that in most of the remaining cases the radius of the isobars was large. We are therefore obliged to neglect

BAROMETRIC GRADIENTS FOR KEW, ENGLAND.

Velocity of Wind.	Gradient for 1°.	G'	G''	G–G''
11·40	3·048	1·772	2·914	+0·134
9·92	2·743	1·546	2·535	+·208
9·83	2·540	1·532	2·512	+·028
8·54	2·235	1·331	2·183	+·052
7·38	2·032	1·150	1·886	+·146
6·70	1·727	1·044	1·712	+·015
5·63	1·524	·877	1·439	+·085
5·19	1·219	·809	1·326	—·107
4·11	1·016	·641	1·050	—·034
3·35	·711	·522	·856	—·145
3·13	·508	·488	·800	—·292
2·23	·203	·347	·570	—·367

the direct effect of centrifugal force, and it is presumed that the error resulting from this omission is not very great. Since the pressure and temperature of the air corresponding to the different wind velocities is not given, we are obliged to neglect the influence of these terms. The latitude of Kew is 51° 28′. Under these conditions Ferrel's formula becomes

$$G = 0{\cdot}1558s,$$

from which are deduced the values in column third. These values (with the exception of the last) are all less than the observed gradients shown in column second. If we multiply each of the numbers in column third by 1·64, we obtain the numbers in column fourth. The differences between the numbers in column second and those in column fourth, are shown in column fifth. It will be seen that for velocities of the wind greater than twelve miles per hour, the corresponding gradients in column fourth are less than the observed gradients; but for velocities less than twelve miles per hour, the gradients in column fourth are greater than the observed gradients. The differences shown in column fifth may in part be ascribed to the factors of the formula which we have been obliged to neglect.

These results confirm the conclusions previously stated in this paper, and appear to demonstrate that the principles of Ferrel's formula are correct, except that the effect of friction is considerably greater than he has supposed.

The preceding paper was written and presented to the National Academy of Sciences last April, but its publication has been delayed by sickness.

ART. LI.—*A Brief Study of Vesta*; by M. W. HARRINGTON.

It has long been suspected that several of the asteroids have variable light, but so far as I know they have not been studied with reference to this point except by Ferguson over thirty years ago. One of the tasks which I have desired to undertake has been that of testing more accurately the variability of the asteroids, and, last spring, while studying the character of observations made with the wedge-photometer, I embraced the opportunity of taking a series of observations on Vesta while near her stationary point. She was at this time not far from Argelander's stars, Durchmusterung, zone 22°, numbers 2163 and 2164, of which the former was fainter, the latter brighter than Vesta. My observations were made by taking on 2164, Vesta and 2163 each, five observations of extinction in as rapid succession as possible. The mean of the five extinctions of each body was taken and is entered as the datum in the following table. The variations in these numbers for the stars are probably due to sources of photometric error, but I took especial pains to have these errors the same for all three of the bodies observed so that they would be eliminated in a comparison of the three. I cannot hope that I have entirely succeeded, but it was my special study to do so.

Sidereal time of observation.			2164.	Vesta.	2168.	Magn. of Vesta.
April, 1883.						
d.	h.	m.	s.	s.	s.	m.
9	XII	17	50·8	39·6	31·6	7·34
13	XII	31	46·4	37·5	25·5	6·79
	XIV	57	38·3	32·4	19·2	6·38
	XV	24	36·2	29·0	16·7	6·59
15	XII	38	44·7	36·0	24·0	6·77
	XIII	19	46·8	38·7	25·8	6·65
	XV	43	41·5	33·9	21·7	6·55
16	X	3	47·8	38·1	27·2	6·95
	XI	55	46·4	37·1	25·6	7·07
	XII	26	47·2	38·8	25·8	6·66
	XII	55	46·9	36·6	25·7	7·00
	XIII	24	44·3	35·2	24·5	6·91
	XIII	49	44·8	35·1	24·9	7·01
	XIV	18	43·4	32·6	22·5	7·01
17	IX	39	44·6	36·0	23·5	6·76
	X	11	49·5	39·9	28·2	6·71
	X	40	48·7	40·1	27·5	6·72
	XI	5	47·5	37·7	27·1	6·98
	XI	29	46·7	37·9	25·4	6·75
	XI	50	46·8	37·9	25·4	6·76
	XII	11	45·2	36·1	24·5	5·84
	XII	31	45·5	37·0	26·2	6·85
19	XII	41	45·3	35·8	24·8	6·92
23	XII	8	47·6	36·5	26·6	7·12
	XII	58	48·3	36·9	26·7	7·15
26	XI	55	50·7	39·2	30·2	7·25
28	XII	27	52·0	40·7	31·2	7·20

In the first column are given the Ann Arbor sidereal times of the middle of the set of observations on the three bodies. In the second, third and fourth are the times taken by the stars from entrance on the wedge to extinction. Argelander gives as the magnitude of the first comparison star 5·3 and of the second 8·8. If we adopt these magnitudes and assume that the stars do not vary during the course of the observations, we can easily determine the magnitude of the asteroid at the time of observation. As the magnitude of the star and the time of extinction in the wedge change as the logarithm of the light, they can be compared directly. The fifth column was formed by the proportion,

$$m_1 - m_2 : m_1 - m_3 :: e_1 - e_2 : e_1 - e_3,$$

here m_1, m_2, m_3 and e_1, e_2, e_3 are the magnitudes and times extinction of 2164, 2163 and Vesta respectively. The change brightness of Vesta due to change in distance from the earth ould not be appreciable from day to day. Indeed, for the hole interval during which observations were taken it would but about 0·25 of a magnitude. It may therefore be neg-cted for the present purpose.

Can we conclude from the observations that Vesta is variable? think that we can. A test of the accuracy of the observation obtained by comparing the difference in time of extinction of ıe two stars. It is fairly uniform and generally decreases at ıe later hours as the stars approached the horizon—as it should o. The errors arising from this source would be large if we ttempted to obtain Vesta's light by comparison with one star, ut they are eliminated by comparing her with two between hich she falls in brightness.

As to the first magnitude obtained (which is the lowest of ll) the record gives no evidence of its being less trustworthy han the others. On the contrary, Vesta was at that time in he best position for comparison, and I had already had some ractice in comparing her with a single star. Her faintness at hat time was so great as to strike my attention before putting he wedge on my instrument. But leaving that out, the bservations show a maximum at about xv^{h} on the 13th, one pproaching on the 15th at the close of the observations, a urious variation on the 16th, and a minimum about xi^{h} on the 8th. In general, Vesta is brighter on the 17th than on the 6th, while as she was leaving opposition she should have een slightly fainter if her light is not variable.

The series of observations on the 16th and 17th were taken ith the hopes of finding some periodicity which might lead o some determination of Vesta's rotation on her axis, but ithout entire success.

A few remarks as to Vesta's albedo may not be out of place. everal attempts have been made to measure her diameter, and hough the measures are inharmonious, they agree among hemselves better than do the different measures of other asteoids—as Ceres and Pallas. The mean of the measures of chröter, Mädler, Secchi, Tacchini and Millosevich, reduced to esta's mean distance from the Sun, as given by Houzeau Vade-mecum, p. 637), I find to be 0″·49. Employing this, sing Zöllner's formula for computing albedo (Phot. Unteruchungen, page 159), and giving Vesta a purely geometrical hase, I find her albedo to be about 0·1 or much like that of he Moon and Mercury. This suggests that she has something f the physical condition of the Moon, which is also suggested y her size. We may note, however, that if her angular diameter at mean distance from the Sun is 0″·49, her diameter in miles must be not inconsiderable. In fact, a simple calculation shows that with this angular diameter the diameter is about 520 miles, making her more than four times as large as Argelander and others made her by assuming her to have the same albedo as Saturn. Indeed, we may doubt if this assumption is justified on general principles. The albedo of Saturn is peculiar and is approached only by that of Neptune. The asteroids are much more likely to be like their nearest neighbors, Jupiter or Mars. Considering their small size, however, and remembering how important a part the size plays in the process of cooling, causing presence or absence of clouds, water, etc., we may consider it more probable that these bodies resemble those other small bodies, the Moon and Mercury.

As a result of my observations and of the other considerations just mentioned, we may, I think, conclude that the present state of knowledge renders the following conclusions probable:

1. Vesta is a body upwards of 500 miles in diameter.
2. She is like the moon in her albedo and therefore probably like her in lacking an appreciable atmosphere and water.
3. To account for the irregularities of her light, we may presume that she has a very rough surface and rotates on her axis. The time of rotation can not be guessed at, but the rapidity of the changes in her light indicates that it is short.
4. What is true of Vesta is likely to be true, *mutatis mutandis*, of the other asteroids.

Subsequent observation may modify these conclusions materially, or may prove that the asteroids are even more dissimilar in their physical characters than they are in the elements of their orbits. Until we are better informed, however, the above may be of interest as the only indications we have of the physical condition of this large family of planetary bodies.

Ann Arbor, Michigan, Oct. 10, 1883.

RT. LII.—*On a New Form of Selenium Cell, and some Electrical Discoveries made by its use;* by CHARLES E. FRITTS, of New York City.

IN the following pages I give, in a condensed form, the iief results brought out in a paper presented by me to the merican Association for the Advancement of Science, at the eeting at Minneapolis in August last.

The new form of selenium cell which I have devised has e following features:

1st. Its resistance can readily be made as low as desired. ome cells have been made having a resistance as low as nine ms. But I have generally used those measuring between 0 and 5,000 ohms.

2d. The light is caused to strike the cell in the same plane general direction as the current.

3d. They are made in any form to be easily handled and ed, and are practical working apparatus, both for scientific searches and technical uses. The cost, when completed, is dinarily about $100 each.

4th. They are far more sensitive to light than any before nown. One cell was cited which had fifteen times as high sistance in dark as in ordinary diffused daylight in a room.

5th. They exhibit other properties in an equally high degree, specified in the paper.

The process of purifying the selenium was first explained, sulting in the separation of the product into a number of rtions, having very perceptible differences of behavior, both melting and annealing. One portion would become soft hile held in the hand, and could not be made to anneal prop- ly at any temperature, while another portion would anneal d become hard at a low heat and could not be melted with- t previously going through the annealing process. Yet both e supposed to be absolutely chemically pure selenium. The nclusion is that commercial selenium is a mixture, consist- g of several distinct portions, which must either be different lotropic forms of the element, or new elements, similar in pearance, chemical reactions and physical properties, yet dis- nctly different in many respects.

I have found the "vitreous" form, or quickly cooled selen- m, to have many different colors even from the same small ece of the chemically pure substance,—varying from very rk brown, almost black, through shades of brown, blue, rple, and violet, to a beautiful rose-red hue. These are dis- ct from the magnificent ruby-red color which vitreous selen- m shows when a very thin sheet of it is viewed by transmit- d light. The granular or crystalline form also varies,—being sometimes of a very light lead color, at others a very dark gray, a violet, a purple, a dull gold color, and occasionally a blue. These colors are caused by heat.

I have tried many different metals and substances as bases for selenium cells, including iron, brass, copper, zinc, tin, lead, platinum, silver, gold, nickel, german silver, aluminum, bismuth, carbon, wood, mica and glass. My preference is for brass, zinc, or iron or copper thinly coated with tin.

Descriptions were given of cells of the forms previously known, especially of the "strip" cell, made for comparative tests with my own form. The sensitiveness to light of cells heretofore made has usually been quite low—one whose resistance would fall to one-half, or 50 per cent change, by exposure to sunlight, was uncommonly good. Probably the average of results would not exceed 10 per cent of change from dark to light. The most sensitive selenium cell on record up to this time was that of Dr. Werner Siemens, which was stated to have a resistance in dark 14·8 times as great as in sunlight. His measurements of this cell are given in table A, annexed hereto. Table B gives several measurements of a cell of my form, and the paper also gave the measurements of a number of other cells of my construction to show their extreme sensitiveness to light.* A lot of my cells, five in number, presented

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* I have just been measuring a new series of selenium cells of my form and find several of them far more sensitive to light than any of those mentioned in my paper before the American Association for the Advancement of Science. Their sensitiveness is so extraordinary that I hasten to put them on record. Brass cell No. 24, measured Sept. 4th, showed twenty-five times the conductivity in sunlight that it had in the dark, and brass cell No. 23, showed about thirty times as much,—the exact ratio being 29·63 to 1.

I have measured the same cells again to-day, together with several others, with the results given in the following table. All the cells were measured "gold anode," i. e., with the current entering the cell at the gold electrode, and with 23 elements of Leclanché battery, except Nos. 22, 23 and 8. The time was between 2 and 4 P. M., sky partly cloudy, sunlight good but not first-rate.

TESTS OF SELENIUM CELLS IN LIGHT AND DARK.

Selenium cell.	Battery.	In dark.	Sunlight.	Ratio.	Date.
No. 24.	23 cells.	32,500 ohms.	1,300 ohms.	25· to 1	Sept. 4, 1883.
24.	23 "	48,500 "	1,100 "	44·09 to 1	" 5, "
23.	10 "	1,600 "	54 "	29·63 to 1	" 4, "
23.	10 "	1,510 "	50 "	30·2 to 1	" 5, "
25.	23 "	15,000 "	750 "	20· to 1	" " "
22.	23 "	1,530 "	80 "	19·12 to 1	" " "
22.	10 "	2,030 "	130 "	15·6 to 1	" " "
6.	23 "	5,040 "	170 "	29·65 to 1	" " "
5.	23 "	2,400 "	150 "	16· to 1	" " "
10.	23 "	60,000 "	3,600 "	16·6 to 1	" " "
17.	23 "	1,790 "	250 "	7·1 to 1	" " "
8.	10 "	30,000 "	1,000 "	30· to 1	" " "

I have a number of others not yet tried, which may give even greater results. But is it not astonishing that the electrical resistance of a substance should be for inspection and trial by the members of the association, was then described.

As the result of some thousands of tests and measurements made by me, I have reached the following new conclusions:

1st. The electrical resistance of a cell changes enormously with different battery powers. There does not appear to be any invariable law governing this change, but each cell seems to have its individual character in this respect. In most cases the resistance becomes lower as the electromotive force of the current increases. One cell cited measured, with one Leclanché element, 14,000 ohms; with 5 elements, 9,900 ohms; with 10 elements, 7,600 ohms; with 23 elements, 4,600 ohms. Another measured, with 23 elements, 3,600 ohms; with 10 elements, 8,000 ohms; with 5 elements, 10,000 ohms. In other cases, but less frequently, the resistance increases as the battery power increases. These changes may be produced in either direction, and as often as desired.*

2d. I have discovered that simply reversing the direction of the current through a cell can make its resistance, in some cases, as much as ten or fifteen times as high as before, even though that increase should amount to millions of ohms. In some instances the change may be even greater, in others not so much, but it is seldom less than twice as much, or as two to one. When the original direction of the current is restored, the resistance also returns, and these effects can be repeated any number of times. The cell is sensitive to light in both cases, but is generally more sensitive when the current enters the selenium at the same surface which the light is acting upon. Instances of such changes were given, and several hypotheses were considered, but none were thought to satisfactorily account for this phenomenon.†

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forty-four times as much in the dark as in the sunlight? I also wish to record the fact that I have constructed a selenium cell whose resistance becomes greater in the light and less in the dark,—being, so far as I know, the first instance of that kind ever obtained.—New York, Sept. 5th, 1883.

* In rare instances, the increase of resistance goes on up to a certain battery power, and any further increase of intensity in the current causes a fall in the resistance,—so that a change from that battery power in either direction would produce the same change in the resistance of the cell.

† The foregoing changes are not always in the same direction. That is to say, if the cell has a certain resistance, with the positive pole of the battery connected to a certain electrode of the cell, the resistance will in some cases be increased by connecting the negative pole of the battery to that electrode, and in other cases it will be lowered. Instances have been found where reversing the direction of the current caused no change in the resistance of the cell. And, what is still more singular, I have had two cells which reversed their action while being experimented with,—so that the electrode which offered the highest resistance to the positive current at first, afterwards offered the lower resistance to it, no change having been made in the connections or conditions. The selenium or the cell seemed to have been in some way permanently affected by the action of the current flowing through it. The cause of this change has not been ascertained.

After some speculations as to whether the foregoing actions are properties of selenium, or are produced by the arrangement of selenium in contact with substances so widely separated from it in the electrical scale, I observed that it is at least evident that the peculiar construction of my cells causes these actions to be manifested many times more powerfully by them than by cells of other forms, and then inquired: If it be possible, by such simple means as are employed in my cells, to obtain these results, may not means be found to still further facilitate these manifestations, and so intensify and exaggerate the results,—and even to obtain others yet unthought of, and possibly still more surprising?

3d. Still I have found that the *kind* of battery employed has a great deal to do with the performance of the cell. Take iron cell No. 1 as exemplifying this. This plate is one of the best, having given a change of 83 per cent, has been used in almost every conceivable way, and always proved good. Yet on removing the Leclanché battery which was usually employed, and substituting the small bichromate plunge battery, this plate became apparently sulky, and finally refused to respond at all to the action of light. Changing from 24 cells to 48 cells of battery has but little effect, whether the gold is the anode or cathode. It is still absolutely insensitive to light.

But on changing the battery to 96 cells its resistance jumps from 265 ohms to 11,500 in dark and 5,000 ohms in light, resuming its old sensitiveness, but apparently under compulsion. On changing to gold cathode, it sinks from 9,000 to 1,750 ohms in dark, then gradually falls to 275 in the dark, and the same in the light. Reversal to gold anode produces no change; it still remains at 275, sinks to 250 dark, and 245 light, and, had another measurement been taken, would doubtless have proved insensitive again. On removing the obnoxious battery for one minute, its resistance springs up to 12,000 ohms in the dark (still gold anode), and 5,000 in light; but gradually falls off again till it shows only 22 per cent change from dark to light. On substituting its old companion, the Leclanché battery, it immediately becomes tractable, and ends up with its original sensitiveness.

Brass cell No. 6, which has given 85 and 88 per cent with Leclanché battery, appeared to be almost worthless with the bichromate cells, showing but little sensitiveness to light,—in one measurement, none at all; and in another, its resistance was actually less in dark than in light,—the figures being 540 and 750 ohms, respectively. But on putting it with the Leclanché, its action changed, it became fairly sensitive to light, and behaved more like its old self.

Brass cell No. 12 also failed with the bichromate battery and became entirely insensitive to light, showing no change whatever between light and dark.

Brass cell No. 5, when connected with the bichromate battery, refused from the first to show any sensitiveness at all. Its resistance varied continually, from high to low, and up again,—most of the time changing too rapidly to admit of getting any measurement. Changing the battery power from 12 cells to 24, 48, and 96 cells made no difference,—it still refused to respond to light, although reversing the current varied the resistance considerably. When one cell of the Leclanché battery was substituted, it started at 10,100 ohms in dark, and 5,700 ohms in light,—nearly 44 per cent decrease in light.

Doubtless other cells would have shown the same action, had there been time to test them, but these are sufficient to excite one's surprise. As before stated, the bichromate batteries had about the same surface in the liquid as the Leclanché. But even supposing that the current of the former had much greater quantity, intensity, or both, we might expect a change of resistance, *but how could it affect the sensitiveness of selenium to light?* If one kind of battery current destroys its sensitiveness, may we not suppose that another kind of battery might increase its sensitiveness? Although the Leclanché has operated well, some other may operate still better, and by its special fitness for use on selenium cells may intensify their actions, and so bring to light other properties yet unthought of. If so, where is the limit to this method of exalting the properties of selenium?

Is not here a promising field for experiment, in testing the various forms of battery already known, or even devising some new form especially adapted to the needs and peculiarities of selenium cells?

4th. The effect of intermittent currents, and of rapidly alternating currents, is usually very slight and may be disregarded in practical work. But occasionally they produce very surprising changes. Sometimes they reduce the resistance of the selenium cell from many thousands of ohms to almost nothing, and at others they raise it from next to nothing up among the thousands. I have not been able to ascertain any connection between conditions and results, but the effects are certainly remarkable. They are not due to my mode of arranging the parts of the cells, for the same thing occurs with other forms, as for example:

Experiment 1. The large "strip" cell measured, in the dark, 1,600 ohms. "Tried intermittent current. Needle erratic,—finally settles at 2,600 ohms." This experiment was with 22 cells of Leclanché. On trying the same experiment on another day, no effect was produced, although the conditions were the same, so far as I could detect.

Experiment 2. Double cell No. 1, measured 50,000 ohms. Put on an automatic reversing apparatus (arranged to both break circuit and reverse the current about 300 times per minute), with 22 cells of Leclanché, for three minutes. The cell then measured, under the same conditions as before, only 30,000 ohms. A repetition of this treatment produced no further effect. The change, produced as described, was permanent.

The action of both intermittent and alternating currents upon selenium are worthy of careful study, for under certain conditions they are capable of effecting great changes in its resistance, which might be utilized for practical purposes if those conditions were understood.

5th. Very moderate changes of temperature (say 10° to 50° Fahr.), can sometimes change the resistance of a selenium cell hundreds or even thousands of ohms in a few seconds, and I think that this phenomenon has not been observed by others.

Experiment 1. Brass cell No. 6 measured, with gold anode, 23 cells Leclanché, in dark, 3,500 ohms. It was laid with its back upon an iron block warmed to about 100° Fahr., when its resistance fell almost instantly to 2,900 ohms, being a fall of 600 ohms.

Experiment 2. The same cell, in the same conditions at another time, measured 3,600 ohms. On putting the hot block under it, it fell to 2,400 ohms; in 30 seconds more it fell to 2,000 ohms; in another 30 seconds, to 1,850 ohms; in two minutes more to 1,600 ohms. By this time the block was but slightly warm.

Experiment 3. A cold block (i. e., not heated), was then put under the cell, when it rose at once to 1,920 ohms; in 30 seconds more, to 2,000 ohms; and continued to rise rapidly to 3,010 ohms. On repeating experiments 2 and 3, similar results were again obtained.

It will be observed that these effects were obtained by very small changes of temperature, and the phenomenon is distinct from the ordinary changes in electrical conductivity observed at different temperatures of the conductors, as well as from those produced by very high temperatures. The fact that an increase of temperature, so slight that the heated block barely feels warm to the hand, can change the resistance of a selenium cell many hundreds and even thousands of ohms, almost instantly, and that the withdrawal of so little heat can produce a change in the opposite direction, almost as great and as rapidly, is one of some importance, and has not been published before, so far as I am aware.

is action is more prominent in brass cell No. 6, than in of the others. The same experiment tried with iron cell . 1 produced no perceptible change in the resistance.

Experiment 4. Double cell No. 1, measuring 33,000 ohms, was laid on warm blocks, and fell instantly 3,000 ohms, then sank rapidly to 22,000 ohms. The same experiment, with double cell No. 2, the mate to No. 1, produced hardly any effect. When the "strip" cell was tried in the same way, it fell slowly, from 1,023 ohms to 955 ohms, in about five minutes. This action therefore appears to be exhibited much more strikingly by cells of my form than by those heretofore made. But why it occurs with some cells and not with others, has not been ascertained. The sensitiveness to light is greater when the cell is cold.

New form of Selenium.—Since the foregoing paragraphs were written, I have been trying some experiments long contemplated, and have succeeded in producing *colorless, transparent selenium.*

It is well known that vitreous selenium when very thin becomes translucent and has a beautiful ruby red color. The new form of selenium just discovered by me has no color, but looks like a thin coating of glass, through which the yellow brass base is clearly seen,—even the fine scratches and marks left in the brass from the polishing being as distinct as if the metal were bare. The mode of obtaining it is such as to exclude everything but selenium. The conditions of its production are these: The exposure of the selenium to a prolonged heating, followed by gradual cooling, under pressure. The plates so made have portions which are transparent, situated apparently where the temperature was highest. The remaining portions of the plates are black and red, interspersed with gray. Thus upon one plate may be seen selenium in four different states or colors. As yet, I have been able to make only a partial examination of one of these plates. A small portion of the transparent selenium having, perhaps, one-half square inch of surface, had a resistance of only three ohms. It was found to be affected by light very little, if at all.

As circumstances compel me, much against my will, to devote most of my time to a widely different branch of science, I do not wish to stand in the way of those who would work out these subjects more rapidly, and therefore offer my results for their consideration, and leave further researches to be carried out by any desiring to do so who may have more time and better facilities than myself for theoretical investigations.

TABLE A.

SELENIUM CELL, TESTED BY DR. WERNER SIEMENS.

Selenium in	Relative Conductivities.		Resistance in Ohms.
	Deflection.	Ratio.	
1. Dark	32·	1·	10,070,000
2. Diffused daylight	110·	3·4	2,930,000
3. Lamplight	180·	5·6	1,790,000
4. Sunlight	470·	14·8	680,000

TABLE B.

SELENIUM CELL, TESTED BY THE AUTHOR.

Resistance in Ohms.

Brass Cell, No. 16.	Test No. 1.	Test No. 2.	Test No. 3.	Test No. 4.	Test No. 5.	Test No. 6.	Test No. 7.
Dark	210·	210·	170·	110·	210·	219·	395·
Diffused daylight	49·	14·	53·	13·	110·	90·	72·
Per cent of change	76·6	93·3	68·8	88·1	47·6	58·9	81·7

Note.—A paper on "The Action of Light on Selenium," by Professor W. G. Adams, F.R.S., and Mr. R. E. Day, in Proc. Roy. Soc., vol. xxv, p. 113, contains the following statements:

1st. That on reversing the direction of the current through selenium the "resistance was always found to be different from that previously obtained." As I have shown, the resistance is not always different.

2d. "The first current through the selenium, if a strong one, causes a permanent 'set' of the molecules, in consequence of which the passage of the current during the remainder of the experiments is more resisted in that direction than it is when passing in the opposite direction." As already stated, this "set" is sometimes in the direction above laid down, sometimes in the opposite direction, sometimes there is no "set" at all, and in two cases the "set" *changed* and was reversed during the measurements of the cells.

3d. "That with the same piece of selenium at the same temperature, the resistance diminished as the battery power increased." This I have found by experiments with more than one hundred cells to be correct in some cases, while the opposite occurs in other cases, and in still other instances the increase or decrease of resistance stops at a certain battery power, and is then reversed.

It will be seen that their experiments and results were entirely different from mine, except on the three foregoing points. As to them, so far as the above quotations agree with my results, those investigators are entitled to the credit of first discovery.

New York City, Aug. 28, 1883.

Art. LIII.—*The Ischian Earthquake of July* 28, 1883; by C. G. Rockwood, Jr., Princeton, N. J.

The island of Ischia is situated at the western extremity of the bay of Naples, about twenty miles in a direct line from Vesuvius. It forms a part of the Vesuvian volcanic district, being an extension of the Phlegrean Fields with which it corresponds in geological character, as does also the smaller island of Procida lying between Ischia and the mainland. Ischia contains about twenty-six square miles, being eight kilometers or about five miles long east and west, by 4·8 kilometers or about three miles broad. In the center of the island rises Mount Epomeo, 792 meters (2,782 feet) high, an old volcano which during historic times has given no lava from its crater, and for over five hundred years has been nearly silent. The last emission of lava from its flanks occurred in 1302, since which time the volcanic activity has been shown only by hot springs and steam jets, and not infrequent earthquakes, the last notable one having occurred March 4, 1881.

Ischia is noted for its thermal springs, which with the pleasant climate make it a favorite summer resort, and during the season the town of Casamicciola is usually crowded with strangers. Other towns are Ischia on the east coast, Forio on the west and Lacco Ameno on the north.

Before the earthquake, which wrought so much damage in this beautiful island on July 28, 1883, there had been some indications of unusual subterranean activity; some of the hot springs had shown abnormal variations of temperature, but it is doubtful whether to an extent greater than at other times when no earthquake followed;—there had been a number of slight earthquake shocks in different parts of the island,—and the instruments in the seismological observatories at Naples and Rome were in increased motion. It is said also that unmistakable indications of an approaching earthquake were observed in Casamicciola, but were concealed from the public so far as possible, for fear of alarming the summer visitors and so diminishing the gains of the hotel keepers. These indications, beginning from a fortnight before the great earthquake shock, gradually increased in intensity and frequency, and Professor di Rossi of Rome said afterward that if there had been in Ischia any system of observation, by which at the time he could have been informed of all these phenomena, "he would not have hesitated an instant in pointing out the imminent danger of an impending seismic disturbance" menacing Casamicciola. But in the absence of observations in Ischia, the indications of the instruments on the mainland were interpreted as being fulfilled by the earthquake of July 25th in Calabria. On July 27, however, the unusual noises before noticed at the Solfatara of Albano had increased to an alarming extent, and numerous slight shocks were felt at Vesuvius and vicinity, indicating that the seismic disturbance was not yet over. The violent shock came about 9.25 P. M. on Saturday, July 28, and the greatest damage was done at Casamicciola and vicinity. This town, built on two small hills on the north slope of Epomeo, was entirely destroyed, only one or two houses being left standing. A performance was in progress at the theatre, and when the building collapsed at the shock many were buried in its ruins. Lacco Ameno, on the coast about three miles northwest from Casamicciola, was also mostly destroyed and Forio was greatly damaged. The town of Ischia, on the east coast, was severely shaken, but without suffering very much damage. But the villages of Fontana and Serrara, situated in the interior of the island, and indeed within the old crater, were great sufferers, as was also Barano more to the south. Two large land-slips were caused on the north slope of Epomeo, but no true fissures were found anywhere, and no apparent changes of level.

The first accounts of the killed and wounded were largely exaggerated, the number of killed being stated as 4000, 5000, 8000 and even 18,000. The numbers given by the official commission (Sept. 26) were 1990 killed and 374 wounded. Many perished beneath the ruins of fallen walls, and some were rescued from such a death after having been thus entombed for longer or shorter periods; in one case several persons were taken out of the ruins alive nearly a week after the catastrophe.

The shocks were at first vertical, then undulatory, and lasted about fifteen seconds. The direction of the vibration is variously stated by different observers.

In endeavoring to trace this earthquake to its cause, we must not overlook the connection of this locality with the Vesuvian volcanic district. It will be desirable also to state briefly the location of the hot springs and stufas or steam jets, for which the island is noted. As we follow then the northern coast from east to west, we find thermal springs at Pontano, Fornello and Fontana near Ischia, stufas and thermal springs at Castiglione, stufas at Cacciuto, all near the northern coast, then the abundant hot springs of Gurgitello near Casamicciola, fumaroles at Monte Cito west of Casamicciola, and finally the springs of Paolone on the slopes of Monte Nuovo near Forio. All these lie on a line slightly convex toward the north, and crossing the island from east to west along the northern slopes of Epomeo. They are considered by L. Baldacci to mark an old line of cleavage, from whence come these manifestations of subterranean activity. He also traces another line similarly marked from north to south; from the hot springs of San Restituta near Lacco Ameno crossing the first line at Monte Cito, almost under Casamicciola, cutting through the old crater of Epomeo, along the precipitous valley of the Scarrupato, where are found the inland villages of Fontana, Serrara, Meropane and Barano, and meeting the southern coast at the place where are the hot springs of Fondolillo and the stufas of Testaccio. The east-west line above indicated might find its prolongation in the volcanic islet of Procida, and in the elevated point which projects from the mainland opposite it and connects it geologically with the Phlegrean Fields. Now these two lines coincide nearly with the localities, where this earthquake shock was felt, and at their intersection stood Casamicciola which was so completely destroyed.

In view of these facts L. Baldacci states his conclusions thus: "That the residual volcanic activity of the island is manifested along two principal fissures, one a curve with its convexity to the north, from the baths of Ischia to Forio, the other directed approximately north-northwest and south-southeast between Lacco Ameno and the stufas of Testaccio; and that the place where Casamicciola stood is upon the intersection of these two lines and, therefore, at the very focus of seismic activity and that it always has been and always will be the locality most liable to be devastated by earthquakes."

But other facts also point in the same direction. H. J. Johnston-Lavis has drawn the isoseismal lines about the focus and finds them to be elongated ellipses having their major axes east and west, the lines lying much closer together in their northern portion than in the southern. The minor axes of these isoseismal ellipses is *nearly in the line* of Baldacci's north and south fissure. Johnston-Lavis states his conclusion thus: "From a careful examination of observed azimuths and angles of emergence, all point to a plate-shaped focus whose strike extends in a line from Fontana just west of Menella to near the beach at Lacco. The plane of this fissure is probably roughly perpendicular to the surface, but may dip slightly toward the east, as the isoseismals are slightly nearer on the eastern side of the seismic vertical, which, as a necessity, is not represented by a point, but by a line on the surface. The rupturing of this plate-like fissure was apparently greatest at a point nearly midway between its extremities."

To conclude, then, it seems to be pretty clearly made out as probable, that this earthquake shock had its origin and cause in a rupture taking place along an old volcanic fissure, directed roughly north and south, and extending radially in or under the northern slope of Epomeo; and that the cause of the increased tension resulting in this rupture is to be referred to the residual volcanic activity which this island shares with the adjacent mainland rather than to any merely local subsidence.

Palmieri has advanced the opinion that the local violence of the shocks might be due to the collapse of subterranean cavities not very far below the surface. But it would seem likely that any subsidence of sufficient magnitude to cause the surface destruction, seen at Casamicciola, and to give rise to vibrations felt by instruments so far away as Rome, must have been attended also by surface fissures and changes of level, none of which were observed.

After the disastrous earthquake of July 28, slight shocks continued to occur, at first daily, and afterward at longer intervals, even as late as September 23d.

In preparing this notice free use has been made of the correspondence published in various journals and newspapers both of America and Europe, but the writer is especially indebted to articles contributed by Ch. Vélain to *La Nature* (August 18, 1883); by H. J. Johnston-Lavis to *Nature* (Sept. 6, 1883), and by L. Baldacci to the Boll. R. Com. Geol. (translated in Science, Sept. 21, 1883.)

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *A method of isolating heat radiation from the luminous and actinic radiations.*—In a note communicated to the French Academy, M. F. Van Assche describes his process, which consists of depositing a thin film of distilled and melted selenium upon a plate of glass which he designates by the term *porte-object.* The selenium is immediately covered with a thin plate of glass called the *couvre-object.* The selenium is pressed into a thin plate by means of heat and by pressure, taking care not to boil the selenium, to avoid bubbles and striæ. The light transmitted through this thin plate of selenium is mono-chromatic, and of a reddish tint. Seen through the spectroscope, the light is comprised between the A line and the C line. The double line B and the numerous small lines *a* are readily seen, but the D lines can not be observed. There is, so to speak, a transformation of the luminous radiation of the sodium lines. A great band of absorption lies between A and C. It appears that the luminous, heat, and actinic radiations are interrupted by the selenium when the source of these radiations has a temperature between 720° and 2000° and perhaps transformed into electrical energy. The radiations due to comparatively low temperatures, 525° to 720°, alone can penetrate the selenium. A thin plate of selenium heated to 250°, can

convert all the radiations into obscure radiations. One can utilize this specific property of selenium in analyzing the heat rays; also in a photographic chamber; in the use of the opthalmoscope, and generally in the analysis of the radiation of the sun and the stars. — *Comptes Rendus*, No. xvi, 15 October, 1883, pp. 838–840.

J. T.

2. *Telluric Oxygen lines.*—M. Egoroff has shown that the lines A and B of the solar spectrum are due to the oxygen in the earth's atmosphere. He employed a tube twenty meters long closed at both ends by glass plates; filled this tube with dry oxygen under a pressure of fifteen atmospheres; observed the light from an oxy-hydrogen flame and found that the lines due to the absorption of the oxygen were identical with A and B in the solar spectrum. The theory that these lines are due to a cosmical hydrocarbon gas diffused through space is controverted by the experiments of M. Egoroff, who found that several kinds of hydrocarbon gas gave no bands or lines of the character of A and B.—*Comptes Rendus*, 27 August, 1883, pp. 555–557. J. T.

3. *A new Capillary Electrometer.*—M. A. Chevet has devised a modification of Lippmann's Electrometer which can be readily constructed and which will show a difference of potential from $\frac{1}{1000}$ to $\frac{1}{10000}$ of a volt. Two flasks with lateral orifices on the same horizontal line are connected through these orifices by the tube of a thermometer open at both ends. The bulb end enters the flask A which is filled with mercury. The capillary end enters the flask B. This latter flask is filled partly with mercury and partly with water acidulated with $\frac{1}{10}$ part of sulphuric acid. The capillary end of the thermometer enters the acidulated water. A platinum wire, P, insulated by a vitreous covering so as not to be in contact with the acidulated water, is in contact with the mercury of flask B. Another platinum wire, N, is in contact with the mercury of the flask A. By means of a commutator a difference of potential can be intercalated between the ends of P and N. The heights of the mercury and water in the flasks A and B are such that P and N being connected by a metal wire, the surfaces of separation of the two liquids is in the region of the capillary portion of the larger end of the thermometer tube. Let α be the angle of the cone which is tangent to the surface of the tube at the point where the meniscus is formed; α is a very small angle. Let a be the capillary depression and r the radius of the tube. Then $a = \frac{M}{r}$ where M is dependent upon the difference of potential intercalated between P and N. The movement of the meniscus is observed with an eye piece. By means of a manometer attachment differences of potential can be compared by the method of Lippmann.—*Comptes Rendus* pp. 669–672, 17 September, 1883.

J. T.

4. *Hall's Phenomenon.*—M. Aug. Righi modifies the form of thin plate used by Dr. Hall; instead of a cruciform plate it has any shape whatsoever and carries three electrodes. The current

enters or departs by one of the electrodes, and departs or enters by the two others. The two partial currents circulate in opposite directions through the two circuits of each of the bobbins of a Wiedemann galvanometer. A suitable resistance having been introduced the needle of the galvanometer can be maintained at zero. When one places this arrangement of partial currents between the poles of an electro-magnet the direction of the deviation of the needle shows that the equi-potential lines are turned in the opposite direction from the magnetizing current—for gold and metals which have the same action as gold—and in the same direction as the magnetizing current for iron. By changing the direction of the current in the plate, deviations of the galvanometer needle are obtained which show that the effect cannot be explained by the direct action of magnetism upon the current. M. Righi promises to explain this in a forthcoming memoir. The action in bismuth is very strong, being nearly 5000 times as strong as in gold, with the same strength of current and the same dimensions. A plate of bismuth of $0{\cdot}079^{mm}$ in thickness gives indications five or six times greater than a piece of gold leaf $0{\cdot}000087^{mm}$ in thickness. The effect can be obtained in bismuth with a permanent magnet instead of an electro-magnet, and the author hopes to be able to show Hall's phenomenon by means of the earth's magnetism. J. T.

5. *Note on the Vortex-Atom Theory;* by JAMES CROLL. The vortex-atom theory, viewed from the physical side, is regarded by some as one of extreme simplicity. I must confess, however, that to me it appears to be far otherwise. In fact, I am unable to understand how the theory is to be reconciled with the first law of motion. According to that law no body possessing inertia can deviate from the straight line unless forced to do so. A planet will not move round the sun unless it be constantly acted upon by a force deflecting it from the straight path. A grindstone will not rotate on its axis unless its particles are held together by a force preventing them from flying off in a tangent to the curve in which they are moving. Centrifugal force must always be balanced by centripetal force. My difficulty is to understand what force counterbalances the centrifugal force of the rotating material of the vortex-atom. It has been said that the centrifugal tendency of the rotating material of the vortex-atom is controlled by the exterior incompressible liquid. But it is also stated that this incompressible liquid offers no resistance whatever to the passage of the atom through it. In short, that in so far as the motion of the atom is concerned this liquid is a perfect void. Now, if this liquid can offer no resistance to the passage of the atom as a whole, how then does it manage to offer such enormous resistance to the materials composing the atom so as to continually deflect them from the straight path and compel them to move in a curve? The centrifugal force of these vortex-atoms must be enormous, for on it is assumed to depend the hardness or resistance of matter to pressure. Now the centripetal force which

balances this centrifugal force must be equally enormous. Then if this perfect fluid outside the vortex-atom can exert this enormous force on the revolving material without being itself possessed of motion, then there does not seem to be any necessity for vortex-motion in order to produce resistance.

It has further been advanced, by way of explanation, that this incompressible liquid surrounds the revolving liquid like a pipe, "and that if the liquid in this pipe were to fly out, a temporary void would be formed in it, which is impossible in a liquid that already occupies all space." The incompressibility of the surrounding fluid surely cannot be a reason why portions of the revolving material do not fly out, for if incompressibility could prevent portions of the revolving atom from flying away, it would equally prevent the whole atom from doing so, but according to hypothesis this incompressible fluid offers no resistance to the motion of the atom. When the atom moves it is assumed that the fluid in front is displaced; but then this simply makes room for an equal quantity behind, and thus no void is formed. The very same thing ought to take place, though only a portion of the atom were to fly off. The various portions of this revolving material are not supposed to be held together by any cohesive force like those of the grindstone. What then prevents the revolving material from being dissipated by the centrifugal force of rotation? In short, how is the existence of the atom possible under the physical conditions assumed in the theory?—*Knowledge, Aug.* 17; *proof communicated by the author.*

II. GEOLOGY AND NATURAL HISTORY.

1. *Geology of the Comstock Lode and the Washoe District;* by GEORGE F. BECKER, U. S. Geological Survey, CLARENCE KING, Director. 422 pp. 4to, with many plates, and an Atlas of 21 sheets in double folio, containing maps and underground sections of the region.—Mr. Becker's Report, after some introductory remarks, treats exclusively of the geology of the Comstock lode, and is a work of great value. Some of its facts and results are here briefly presented.

Tables prepared with care by Mr. Eliot show that this so-called *Silver* lode has afforded up to July, 1880, (or in 21 years) about 43 per cent of gold to 57 of silver, the whole value of the two about $306,000,000.

The rocks of the region (arranging them according to the predominant feldspar in their constitution as made out by Mr. Becker) are as follows:

(1) *Having the feldspar portion chiefly orthoclase:* Granite, a small area near the Red Jacket mine; *quartz porphyry* (dacite of Zirkel), having a microlitic fluidal ground-mass. (2) *Having the predominant feldspar oligoclase: metamorphic diorite,* G $=2·96$ (a variety of basalt, Zirkel) a blackish rock in the southern part of the district. (3) *Having the predominant feldspar labrado-*

rite or andesine: eruptive diorite, widely distributed, often quartziferous, having porphyritic and granitoid varieties, and some mica with the hornblende, the mica occasionally exceeding the hornblende in amount; *Diabase*, the *older*, forming the east or hanging wall of the lode, the *younger*, known as "black dike," extending horizontally more than a mile through some of the mines, and resembling the trap of New Jersey, neither of the kinds having a microlitic or glass-containing ground-mass; *andesite*, having a microlitic ground-mass and often a fluidal structure, including *earlier* and *later hornblende-andesite*, and also some *augite andesite* which differs mineralogically only in the proportion of augite, the *later* hornblende-andesite often called trachyte; and *basalt*, consisting chiefly of labradorite, augite and olivine.

Magnetite and titanite are present in most of the rocks; mica, in much of the diorite and some diabase; zircons, in the granite, quartz-porphyry, diorites, and rarely in the andesite; some hornblende, and rarely mica in the diabase. The granite and diorites are the older of the rocks. After these, the chronological order deduced is as follows: Quartz-porphyry, earlier diabase, later diabase, earlier hornblende-andesite, augite-andesite, later hornblende-andesite, basalt. The most abundant rocks are the andesites. The microscopic character of several of the rocks is illustrated on four plates of sections.

The "propylite" of von Richthofen was proved to be an altered rock, either andesite or diabase.

The rocks adjoining the lode are altered to great depths, and fresh specimens of several kinds are unobtainable. They are often chloritic from the change of augite, hornblende or mica; sometimes contain epidote, which is referred to the alteration of chlorite; calcite, from the lime of the feldspars and exotic CO_2; uralite, from the change of augite; and pyrite, from different sources, the iron coming from one of the iron-bearing silicates. Among the results of weathering, freezing is stated to throw off sometimes conchoidal chips from the angles of andesite blocks to a distance of two or three feet which weigh three or four pounds. The quartz porphyry is deeply decomposed, and its porous structure is said to be "perhaps due to the unequal contraction of the quartz and feldspar in cooling."

The changes along the lode are attributed to waters holding carbonic acid or alkaline sulphides, or other mineral ingredients. The conclusion is drawn, based on assays of the rocks, admitted to be not altogether satisfactory, that the diabase, making the east wall of the vein (and now exposed for a length of 8,000 feet and a thickness of 2,500 feet, and deeply decomposed), may have been a chief source of the gold and silver; that it is probable, from Professor Sandberger's investigations that the augite of the diabase was the seat of its metallic contents; and that sulphydric and carbonic acids were the active reagents that extracted and gathered in the ores of the metals. By their action on the rocks, "carbonates and alkaline sulphides would be formed, and

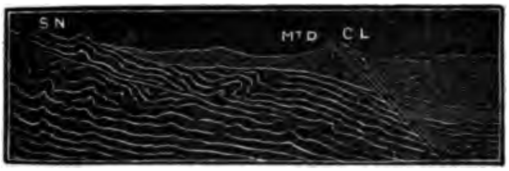
.re solvents for quartz and sulphides of the heavy metals. eated depths of the mine are supposed to have formerly :d sulphydric and carbonic acids for the decompositions ıe filling of the vein, though now affording only a mere ·f these gases. The assays of the rocks adjoining the vein ıostly from 0·03 to 0·21 for the percentage of the precious reckoned as silver.

theory sets aside any supply of ore-material from below exıch as may have come up as a constituent of the diabase, ɔstly of the augite of the diabase. The mine is on this view lent at all depths for its value on the existence of the ad-; diabase, and hence "exploration," Mr. Becker observes, d be confined to the neighborhood of this contact," and .d this rock narrow to a mere dike between diorite walls, tlook would be gloomy."

very simple theory has doubts about it because nothing is of all that happened, in the course of the vein, from the f the ejection of the diabase or the first opening of the fiswhen the temperature was that of the fused diabase—down time when it was 500° F. and less; nothing as to the nature rising vapors at this high temperature, when, because of the rature and pressure, they would have great decomposing .rrying powers; nothing as to whether the wall of the great may not, deep below, have contained metalliferous veins s, or disseminated ore, that would have yielded the metals ir ores easily to the powerful agents of extraction, those heated vapors; nothing as to whether these sources of may not have contributed to the diabase and other rocks ıces of precious metals indicated by the assays.

heat of the mine is proved to increase downward, at the f about 3° F. for each additional hundred feet. "On the 'oot level"—to which depth the country is honey-combed ›assages—"floods of water have entered the mines at 170° ›t enough to cook food. The equation deduced from the facts 'or a temperature of 212° F. a depth of 5,200 feet; and "a ; heat is likely to be struck at any time after passing the 'oot level," and in all probability short of 5,000 feet.

facts show that the source of the heat "can hardly be less ʀo miles from the surface, and is probably four, in short at ›nic distance. In the Sutro tunnel "the rise of temperature lode was approached is best expressed by a geometric showing that the heat is derived from the lode, the moistaqueous vapor present enabling the rocks to conduct heat.



In addition to the escaping aqueous vapor, about 7,000,000 tons of hot water are now yearly pumped from the lode, all of subterranean source but probably originating in the high country of the Sierra Nevada, as the preceding ideal section illustrates.

The question as to feldspar-decomposition (resulting in the making of kaolin) being the source of the heat, as urged by Church, is discussed, and made the subject of careful experiments by Dr. Barus; the conclusion reached is that the amount of heat thus derived is infinitesimal, or not enough to be detected by an apparatus sufficiently delicate to register 0·001° C. Further it is shown that the so-called clay or kaolin is chiefly pulverized rock.

Mr. Becker's Report gives details also with reference to the position of the lode and its contents, leaving much however to be presented to those interested by the fine maps and sections of the folio atlas. It treats also of the structural results of faulting. Besides, it gives the details of an electrical survey of the Comstock and Eureka mines, in which the former afforded only negative results, and the latter a slight action, perhaps enough to be of use in prospecting, especially where the ores are sulphides.

2. *U. S. Geological Survey: Field Work in the Great Basin.*—The division of the Survey to which the Great Basin was assigned, has been employed for several years in a study of the system of lakes which in Quaternary time occupied that desert region. During the past season the professional corps has consisted of Mr. G. K. Gilbert, geologist in charge; Mr. Israel C. Russell, assistant geologist; Mr. W. D. Johnson, topographer; and Ensign J. B. Bernadon, U. S. N., general assistant. Mr. R. Ellsworth Call of David City, Nebraska, is not permanently connected with the work, but has been and is still engaged in the study of the recent and Quaternary shells of the Great Basin, and he accompanied the field parties for a short time. Mr. Gilbert spent a month with Mr. Call in the basins of Humboldt and Pyramid Lakes, and then proceeded to the Mono Basin where the remainder of the party had been at work from the beginning of the season. Mr. Russell has had charge of the investigations in the Mono and Lahontan Basins, and has this year completed his field examinations. The chief part of the summer was spent in the Mono Basin including the adjacent eastern slope of the Sierra Nevada, and the most important subjects of study were the ancient expansion of Mono Lake, and the ancient glaciers of the Sierra. Some attention was also given to the ice-masses which still cling to the northern slopes of the higher peaks, and the camera will be made to give its evidence as to their title to the name of glacier.

All have now returned to the east except Mr. Johnson, who, having completed a general survey of the Mono Basin, remains to make topographic sketches of certain local features, especially of ancient moraines. The Survey has determined to discontinue this investigation of the Quaternary lakes of the Great Basin. The study of the most northern lakes is practically complete, but it has been found impracticable to carry the work southward at present, on account of the great draft on the energies of the Survey incidental to the recent extension of its field. A preliminary report on Lake Bonneville was published in the Second Annual Report, and has been noticed in this Journal. The Third Annual Report will contain a preliminary essay, by Mr. Russell, on Lake Lahontan, and the Fourth, a brief description of the district north of Lake Lahontan, where there was a group of smaller lakes. These are now in type, and the preparation of the final memoirs is well advanced.

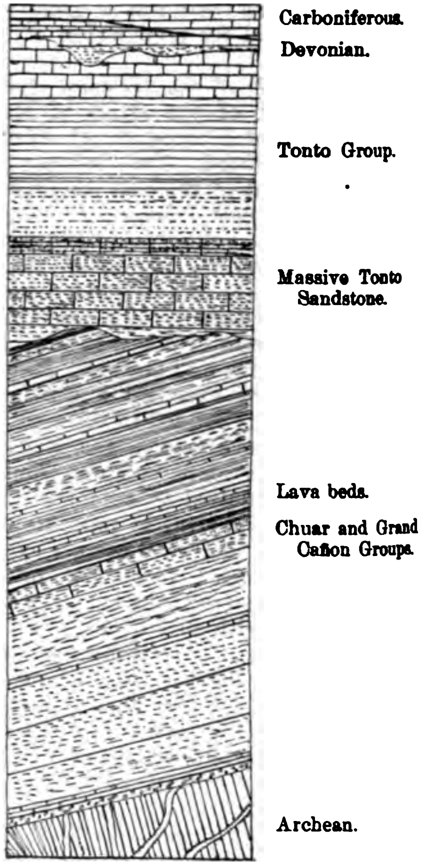
3. *Vol. VII of the Geology and Paleontology of Illinois.*—Volume VII of the series of Reports on the Geology and Paleontology of Illinois, A. H. WORTHEN, State geologist, has recently been issued. The first part, by Mr. Worthen, treats of the development of the coal resources of the State since the publication of the last volume in 1875, and covers 62 pages. The remainder of the volume, over 300 pages, is paleontological. Over sixty species of fossil fishes are described by O. St. JOHN and A. H. WORTHEN, and illustrated on twenty-six plates, crowded with figures. Descriptions and plates of many invertebrate fossils, mostly new crinoids, follow, Part I, by A. H. WORTHEN and S. A. MILLER, and a second part, by C. WACHSMUTH and W. H. BARRIS. The excellent figures are mostly from drawings by C. K. Worthen. Added to those of previous volumes, the whole number of plates issued in the Reports of the Illinois Survey is now 196. Two more volumes, containing forty to fifty plates each, will be required to complete the paleontology. The series of volumes has no rival in paleontological value among the State reports, if those of New York are excepted.

4. *On the recent formation of crystals of Cerussite.*—M. LACROIX has described the occurrence of several copper minerals in connection with pieces of Roman money found among some ruins in Algeria. The coins, as found, adhered to each other, being cemented together by carbonate of copper; between each pair there was a little cavity containing crystals of cerussite and cuprite, with malachite and azurite, but varying according to the degree of alteration which the coin had undergone. The cerussite was in small mammillary crystalline groups, of a yellowish-white color; the cuprite formed red cubes up to 1^{mm} in thickness; the azurite was also in a crystalline state, but without distinct form. The composition of the coins was obtained, as follows: Cu 79·76, Pb 16·26, Sn 3·97=99·99. Somewhat similar observations have been made by Daubrée on recent minerals occurring at Bourbonne-les-Bains, but in the latter case the cerussite was formed directly from fragments of lead, and not as here, at the expense of the lead in the coin alloy.—*Bull. Soc. Min.*, vi, 175.

5. *Volumes I and II of the Reports on the Geology of Wisconsin* have recently been issued. They are volumes of great geological importance. A notice of them is deferred.

6. *Supposed Glacial striæ on Locust Mountain, Pa.*—On page

473, of the last volume of this Journal, the observation that glacial striæ occurred on Locust Mountain, south of the glacier limit, is cited from remarks by Prof. Lesley in the geological report of Mr. I. C. White. We learn from Prof. H. C. Lewis, that he has made a special examination of the region and finds that the supposed glacial striæ are not of glacial origin.



7. *The Pre-Carboniferous Strata in Arizona*; by C. D. Walcott.—The following cut represents a section, by the author, of the Pre-Carboniferous strata in the Grand Cañon, in illustration of his article on pages 437–442. Owing to the dip of the strata, a portion only of the Pre-Tonto groups is shown.

8. *Notes on a new Topaz Locality*; by Rev. R. T. Cross, Denver, Colorado.—In the October number of the Journal for 1882, Messrs. C. W. Cross and W. F. Hillebrand, of the U. S. Survey, described some topazes of unusual size found near Pike's Peak. The locality was in the disintegrated granite on one of the steep slopes about four miles from Manitou. Only a few crystals were found, less than half a dozen good ones, and perhaps twice that number of phenacites.

During the summer of 1882, Mr. Walter B. Smith, a collector, was hunting for minerals in the foot-hills twenty five miles north of Pike's Peak, a region largely destitute of ores and hence not much traversed by prospectors, when he found, near Platte Mountain, a "pocket" of topaz crystals. After digging more or less for a month he secured over a hundred crystals and cleavage fragments. A large number have come into the possession of the writer. The pocket was found in decomposed albite. In the near vicinity is found pegmatite or graphic granite. The following minerals were found in the pocket or very near to it: microcline, albite, clear and smoky quartz crystals, an altered mica, gœthite and fluorite. The largest microcline crystal found measured eighteen inches in the longest diameter. Many of the topaz crystals have been broken—being found out of place—but they show an attempt on the part of nature to heal over the broken surfaces by forming new planes. Some of the microcline crystals show the same peculiarity. The largest topaz fragment weighs eleven and one-half ounces and parts of it are very clear. It evidently belonged to a very large crystal, only two planes of which appear on the fragment. Another fragment, weighing two and a half ounces, is perfectly limpid and colorless. The largest and best crystal weighs four ounces. It is of a light straw color and is perfectly clear, having no flaw in it. The prism is perfect, showing the planes I, i-2, and $\breve{i}$-2. O is distinct, though slightly bruised. Domes, $\frac{1}{2}$, 1, $\frac{2}{3}$, 2-$\bar{i}$, 2-$\breve{i}$ (the latter passing probably into $\frac{4}{3}$-$\breve{i}$), 4-$\breve{i}$, and $\frac{2}{3}$-$\bar{i}$.

9. *Jeremejeffite and Eichwaldite.*—The chemical characters of the remarkable new borate of alumina from Siberia, called by M. Damour *jeremejeffiite*, have already been given in this Journal (xxv, 478). The crystalline form of the species has since been described by Websky. An optical examination by him has confirmed the observation of Jeremejef, made in 1869 upon the same crystals, which were then supposed to be beryl, viz: that only the outer shell is optically uniaxial, while the interior portion is made up of six segments, all alike biaxial. Websky found the two parts of the crystal to be separated by a narrow nearly opaque hexagonal ring. The boundary lines of the six interior segments are perpendicular to the prismatic planes. The optical characters of all the segments correspond, the bisectrix being parallel to the vertical axis, and the plane of the optic axes making an angle of 30° with the adjacent sides of the exterior hexagon. The two parts of the crystal are morphologically different, and to the uniaxial part Websky proposes to restrict the name of Damour, while to the interior part he gives the name *Eichwaldite*, from that of the Director of the mines at Nertschinsk, who collected the crystals. A study of the crystalline form has convinced Websky that the prismatic planes and the pyramidal terminations correspond to the pyramidal-hemihedral division of the hexagonal system, while on the rounded extremities there also occur planes which belong to the interior *Eichwaldite*, and which are to be referred to a *drilling* of the orthorhombic system in accordance with the optical character. It is to be regretted that the material is too scanty to allow of fully deciding these complex crystallographical relations, as also to show whether both parts of the crystals have the same composition. This indeed seems probable, and it is natural to think of the well-known cases in which the same chemical substance is known to appear in both the hexagonal and orthorhombic (or monoclinic) forms; the observations of Professor Cooke on the iodides of antimony are to the point.—*Ber. Ak. Berlin,* June 14, 1883.

10. **Mineralogical Notes**; by E. Claassen. (Communicated).

1. *On [illegible] crystals of Pyrite.*—These crystals occur in a [illegible]-gray shale, the so-called Cuyahoga shale of the Waverly group, at Parma, Cuyahoga county, Ohio. They form combinations of the cube, octahedron and generally the pyritohedron [illegible], with the cube predominating. The largest crystals found measure about ¼ inch in diameter. The interest of these crystals consists in the fact that the cubic planes have always a concave surface, while the planes of the other forms are as even as usual. In consequence of this fact the combination-edges of the cube and octahedron are often curved lines. A single crystal among those examined was peculiar in that the octahedral planes consisted of a depressed triangular portion with an even surface, bounded by three narrow raised parts; the cubic planes have, with one exception, a concave surface; the pyritohedral planes are divided into two parts by a narrow depression. It seems probable that the planes of the original crystal, in this and other cases, were even, and that the concavity and other irregularities observed are due to subsequent growth of the crystal over this nucleus.

2. *On Magnetite-crystals in Hematite.*—Small perfect crystals of magnetite are found imbedded in a compact hematite of metallic luster, brought from the Lake Superior Iron Mines. They are octahedrons, have a red streak and give a red powder exactly like the surrounding hematite, but differ essentially by their being attracted by the magnet. Their appearance is that of unaltered magnetite and not that of martite, which often also occurs in the above named region.

3. *Vanadium and Titanium in Magnetite in the so-called Chaffey ore.*—This magnetic iron occurs near Newboro, Canada, and contains $\frac{1}{33}$ per cent of vanadic acid and 9¼ per cent of titanic acid. The vanadic acid was determined in a quantity of five pounds of pulverized and finely bolted ore, and separated as barium resp. ammonium vanadate.

11. *Phytogeogenesis: Die vorweltliche Entwickelung der Erdkruste und der Pflanzen in Grundzügen; dargestellt* von Dr. Otto Kuntze. Leipzig, 1884. pp. 213, 8vo.—This curious treatise, on the original development of the earth's crust and its vegetation, is of a highly hypothetical character. Much of it we must leave for the geologist to characterize and to digest, as best he may, for instance, that our globe was never an incandescent liquid, nor melting hot, but only moderately red-hot; the oceans formed after sufficient cooling were strongly impregnated with carbonic acid in the form of bicarbonate of calcium, but scarcely salt. The [illegible] disintegration of the land surface gradually gave the sea its saltness. The vegetation which flourished on the surface of this highly carbonated sea gave to the air its stock of carbonic acid. All vegetation down to a comparatively late period—[illegible] down even to the Tertiary—was aquatic and marine; the [illegible] of the Carboniferous period, the Lepidodendra and other [illegible] cryptogams, formed floating islands of vegetation, their

Stigmaria roots floating on the surface in matted masses, their stems rising like masts into the air. As the ocean grew salter this Carboniferous vegetation took to the strand; and then, or subsequently, the vegetation of the world was in peril and verged to extinction, as may be seen from the fact that there were 2500 species of coal plants, and only 150 in later periods. It would seem that increasing saltness of the ocean had driven the higher cryptogamic vegetation to the land before the continents were prepared to receive it; but finally, when the atmosphere had acquired from the bicarbonated ocean sufficient carbonic acid and the rocks had worn down into soil, terrestrial vegetation was established. In the ninth period, heretofore called the Tertiary period, land-plants came to prevail, and the differentiation of climates gives rise to differentiation of land vegetation. Man now appears, in all races probably first with black skin. As there are no indigenous blacks in America, the early population of this continent was an immigration from the old continents. To go back to the beginning: the earliest living things, *Protista*, neither animals nor plants distinctively, were neither miraculously created nor imported from other worlds; but the primordial cells were formed by a kind of mechanical precipitation, in a way which Traube's artificial cells explain. Life arose from certain catalytic processes in carbo-hydrates: only some one of these, with tannin or something of the sort which may precipitate it from a solution, seem to be necessary to cell-formation and growth, and consequently for propagation by division. Sexual propagation in the first instance was probably a morbid process, a sort of diseased action. The two or three primordial organic carbohydrates needful to life originated from certain inorganic carbohydrates of the earth, and these, from interplanetary space. Dr. Kunze is a botanical writer of no small pretension and learning, and is fond of treating his topics speculatively. It may suffice to call attention to his volume, which is replete with suggestion.

A. G.

12. *Catalogue of the Phænogamous and Vascular Cryptogamous Plants of Worcester Co., Massachusetts;* by JOSEPH JACKSON. Worcester. Published by the Worcester Natural History Society, 1883. pp. 48, 8vo.—"Worcester County extends across the State from North to South. On its southern or Rhode Island margin it is allied by its flora to Southern New England; on its northern or New Hampshire margin it is allied to northern New England. It lies at altitudes varying from 200 feet to 2,500 feet above the sea level. Only one point, Mount Wachusett, reaches the latter height. . . . It is a thickly wooded region, with many kinds of valuable timber trees, such as the white pine, white ash, white oak, hickory, chestnut, rock maple," etc.

A faithful list of the plants of such a typical slice of New England, besides its local uses, is interesting and instructive, as giving a fair idea of New England vegetation apart from the influence of the sea on the one hand and the alpine and subalpine heights on the other. "No attempt has been made to distinguish between indigenous and introduced plants,"—which is rather to be regretted from our point of view. One likes to see the native as clearly distinguished from the foreign elements as well may be. For such a district this may now be done quite clearly. In the future it will be more difficult. The Catalogue "contains 812 species and well-marked varieties, distributed among 387 genera;" and [illegible] species are found in Millbury," the author's own residence. The Catalogue is well edited and handsomely printed. We have detected only a single misprint (and that on the last page), besides the wrong use of a capital initial in one instance, which probably is also an error of the press,—a correctness rarely attained in a first essay. There is a list at the end of the "trees, shrubs, and evergreen flowering plants growing naturally in the county, [illegible] in number; also of twelve trees, none which are common in [illegible]." A. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Ice of Greenland and the Antarctic.*—Dr. JAMES Croll has a paper in the Philosophical Magazine for November, opposing the view that the universal ice-covering of Greenland is a consequence of the elevation of the land. He states that no [illegible] mountain ranges have been reported, and holds, as urged by Dr. [illegible], one of the highest authorities in matters [illegible] that the region of the great interior ice- [illegible] shallow vessel, filled with the glacier as [illegible] and pouring portions out through breaks [illegible] also accepts as most probable the opinion, [illegible] by Giesecke, and more recently by Dr. [illegible] country is really a collection of islands fused [illegible]

[illegible] argues that the Greenland condition is probably [illegible] a collection of rather low islands "bound [illegible] sheet of ice," as Sir Wyville Thomson [illegible] space of about 4,500,000 square miles. This [illegible] elevation is stated to follow from the [illegible] structure of the Antarctic ice-barrier. [illegible] bands indicate, by their number and [illegible] long period of annual deposition of [illegible] barrier is probably removed hundreds [illegible] dispersion, especially since there are [illegible] a former glacier-condition, such as [illegible] valleys or fiords. The layers con- [illegible] gradually thinner downward; and [illegible] of compression and melting, is [illegible] increasing breadth of the earth [illegible] requiring that a square foot [illegible] should occupy thirty square feet [illegible] and hence be only $\frac{1}{30}$th of a foot thick.

Hence relative thickness is an indication of distance traveled, and "a layer near the bottom may have been traveling from the Pole for the past 10,000 or 15,000 years," while one at top is not twenty years old, and of short distance of travel.

From such facts Dr. Croll argues that the ice of the Antarctic ice-barrier moved off from a low and even land-surface, and that this kind of surface characterizes the "Antarctic continent." On the ground that the moist winds flow in all directions toward the south pole, and that the latitudinal extent diminishes poleward, it is urged that the loss by precipitation on going poleward does not necessarily occasion a diminishing depth of ice. For if the annual amount of precipitation between the parallels of 60° and 80° is thirty feet, and the amount of moisture left in the air on reaching the line of 80° is only a tenth of thirty, this tenth would give a snowfall there of twenty-four inches, because the area between the line of 80° and the pole is only an eighth of that between the parallels of 60° and 80°. Hence even if the border of the Antarctic ice-region takes from the clouds the greater part of their moisture, the thickness of the ice may still increase poleward. Moreover the region of the pole should be ever filling with ice, however small the precipitation.

Dr. Croll regards the recent observations of Nordenskiöld as confirmatory of his views on Arctic and Antarctic ice-regions.

2. *National Academy of Sciences.*—At the meeting of the Academy held at New Haven, November 13–16, 1883, Professor O. C. Marsh, President, the following papers were presented:

A. GRAHAM BELL: Upon the formation of a deaf variety of the human race.

C. A. YOUNG, E. S. HOLDEN and C. S. HASTINGS: On the Solar eclipse of May 6th, 1883.

ASAPH HALL: Notes on the mass of Saturn.

R. H. CHITTENDEN: On some new primary cleavage forms of albuminous matter. *By invitation.*

S. NEWCOMB: On the use of the word "Light" in Physics; On the theory of errors of observation and probable results.

O. T. SHERMAN: Personality in the measures of Venus's diameter as derived during transit across the Sun. *By invitation.*

E. LOOMIS: The reduction of barometric observations to sea-level.

S. P. LANGLEY: Atmospheric absorption.

W. H. BREWER: On the subsidence of particles in liquids.

H. A. ROWLAND: On a new photograph of the Solar Spectrum.

J. D. DANA: On the stratified drift or terrace formation of the New Haven region, including its kettle holes and deserted river channels.

W. GIBBS: Preliminary notice of phospho-vanadates, arsenio-vanadates, and antimonio-vanadates; On the probable existence of new acids of phosphorus.

B. SILLIMAN: Notes on the mineralogy and lithology of the Bodie Mining District of California.

J. S. NEWBERRY: On the ancient glaciation of North America.

J. W. POWELL: Marriage Institutions in Tribal Society.

J. HALL: Note upon the physical aspects of the higher members of the Chemung Group and the development and distribution of the fossil genera Ptychopteria and Leptodesma, preceded by a review of the Pectenidæ and Aviculidæ of the Devonian system.

T. STERRY HUNT: The Animikie rocks of Lake Superior.

O. C. MARSH: On the affinities of the Dinosaurian Reptiles.

3. *Signal Service Professional Papers.*—The following papers have been issued by the Signal Service. No. VIII. The motions of fluids and solids on the earth's surface, by Professor WM. FERREL, reprinted with notes by Frank Waldo.—No. IX. Charts and Tables showing geographical distribution of rain-fall in the United States, based on observations from the establishment of the Meteorological Bureau of the Signal Service, in 1870 to January, 1881, by H. H. C. DUNWOODY, 1st Lieut. 4th Artillery, acting signal officer.—No. XI. Meteorological and Physical Observations on the East Coast of British America, by O. T. SHERMAN.—No. XII. Popular Essays on the Movements of the Atmosphere, by Professor WM. FERREL.

OBITUARY.

JOHN LAWRENCE LECONTE.—Doctor John L. LeConte, the able Entomologist, died on the 15th of November, at Philadelphia, in his fifty-ninth year, having been born in New York on the 18th of May, 1825. He was a son of Major John E. LeConte, a distinguished member of the Engineer Corps and the author of several papers on Botany and Zoology. Doctor LeConte's labors in science were devoted especially to the Coleoptera, in which department he made very large collections, and was high authority. He is the author also of some mineralogical papers, and as early as 1847 presented to the Association of American Geologists and Naturalists a valuable paper on Fossil Mammalia from Illinois. The Smithsonian Institution published in 1861–62 his *Classification of the Coleoptera of North America*, and in 1863–66 his *List of the Coleoptera of North America*. He was a member of the National Academy of Sciences, and, for the year 1873–74, President of the American Association.

LEONARD D. GALE.—Dr. Leonard D. Gale died in Washington on October 22, in his eighty-fourth year. He was graduated from the College of Physicians and Surgeons, in New York City, in 1830, and was soon afterward employed as Assistant Professor of Chemistry in that institution. His name is connected with chemical papers in this Journal for 1831 and 1832. Later he was Professor of Chemistry in the New York College of Pharmacy, and Professor of Chemistry, Geology, and Mineralogy in the University of New York. While holding the latter position, in 1834, he rendered important aid to his fellow-Professor, S. F. B. Morse, in the perfection of the electric telegraph, bringing to his knowledge the discoveries of Professor Henry, the application of which to the Morse machine assured its success. In 1838, Dr. Gale made a geological survey of New York island, and a report of his work is published in the New York Geological Report (4to, 1843) of Professor Mather. From 1846 to 1857 he was an Examiner of Patents, having charge of the Department of Chemistry, and was afterward for many years engaged in the practice of patent law in Washington.

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ERRATA.

Page 177, line 4 from bottom, add at the end of the sentence, except the violin family.

Page 179, line 1 from top, for 2Vh, read $2V_h$.

Page 181, equation (13), all the coefficients should be 2. Equation (14), all the signs should be alike, viz., +.

Page 183, line 17 from bottom, for These familiar, read Then familiar. Line 8 from bottom, after û insert a comma, and dele comma after (ü).

Page 185, line 14 from top, after velocity add, of the tip of the prong.

Page 187, Table V, column 6, for 2·5, read 1·3.

Page 189, last line, for 1862, read 1882.

Page 217, Figure 5 represents a crystal of allanite, and was inserted by mistake.

Page 279, line 18 from top, for pyramids, read fragments.

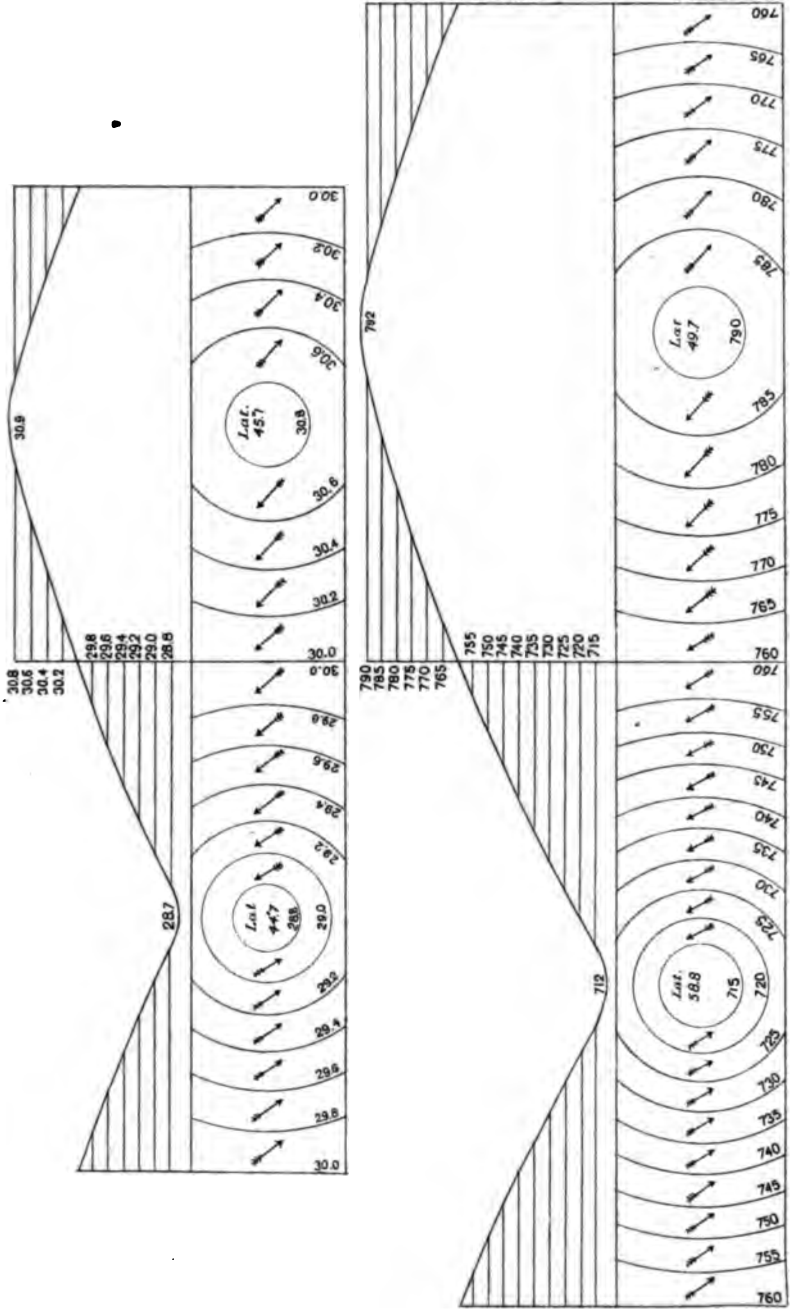
Page 286, line 4 from top, before oxygen dele the.

Page 290, line 17 from bottom, for decomposition, read deposition.

Page 306, line 12 from bottom, insert vol. xxiii at the beginning of the line.

Page 307, line 11 from bottom, for focus, read locus.

BAROMETRIC GRADIENT PLATE I.



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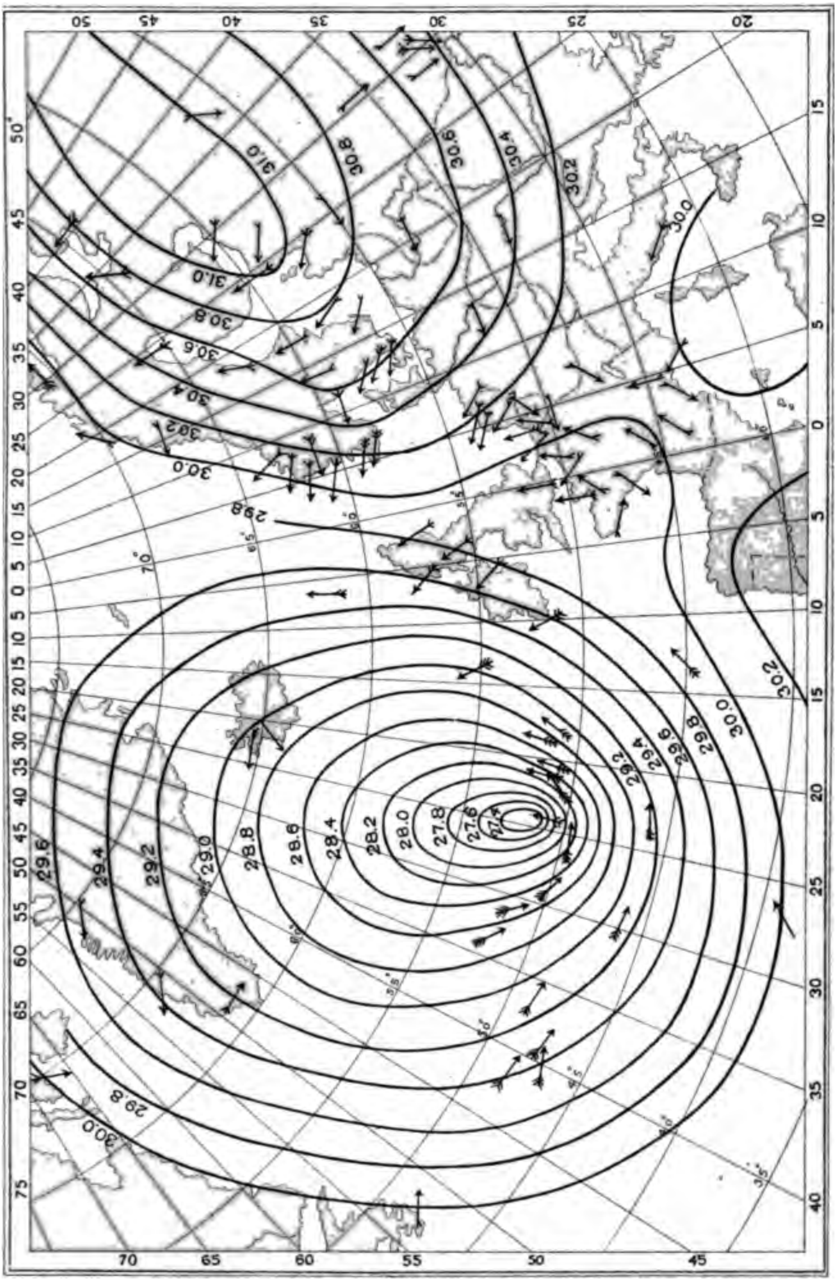
STORM OF FEB. 5 1870. PLATE II.



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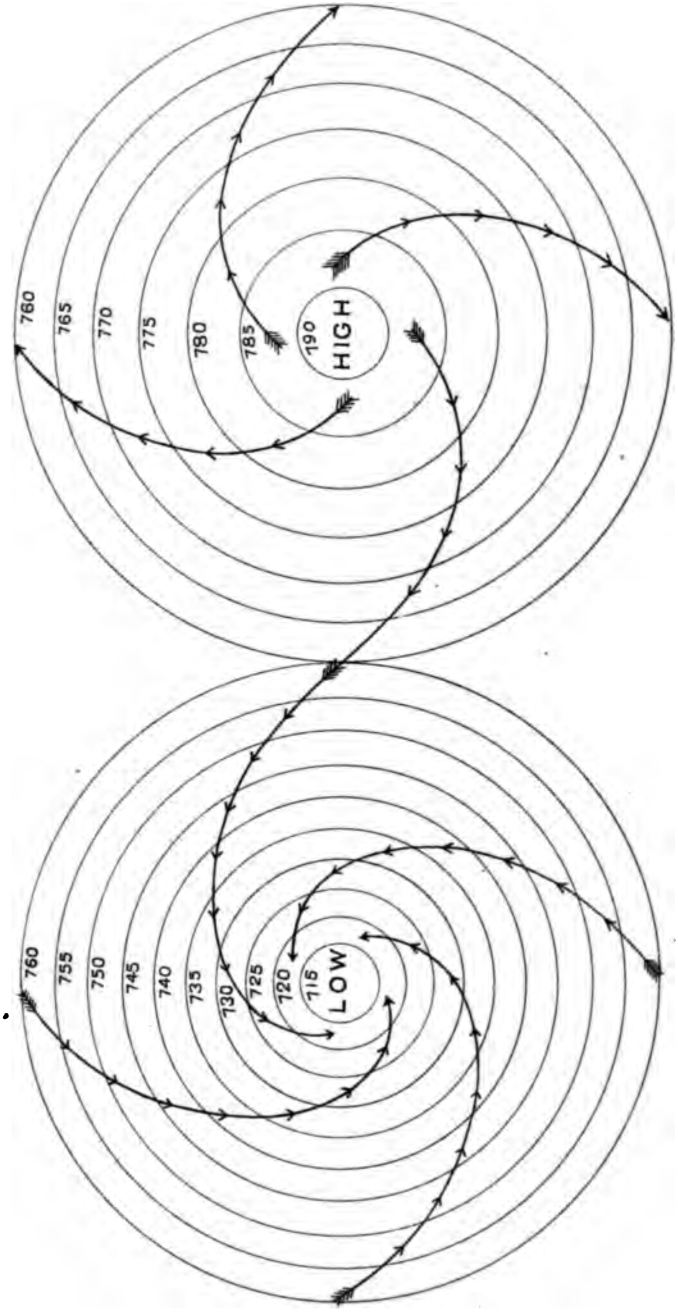
SPIRAL MOVEMENT OF THE WIND. PLATE III.



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